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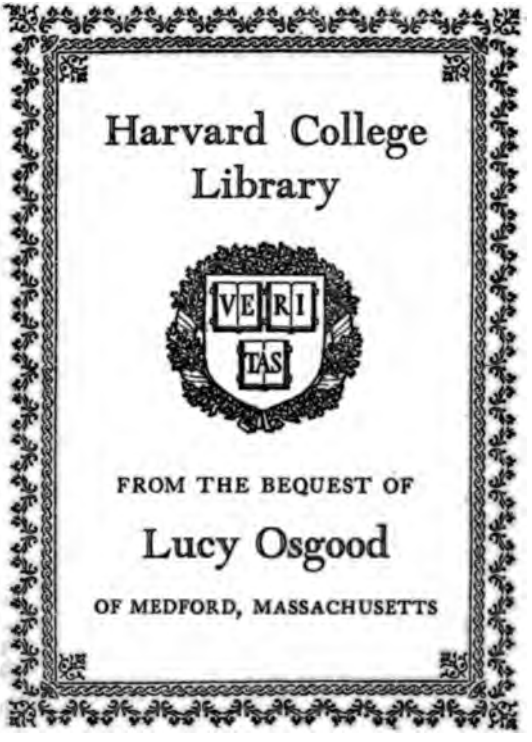
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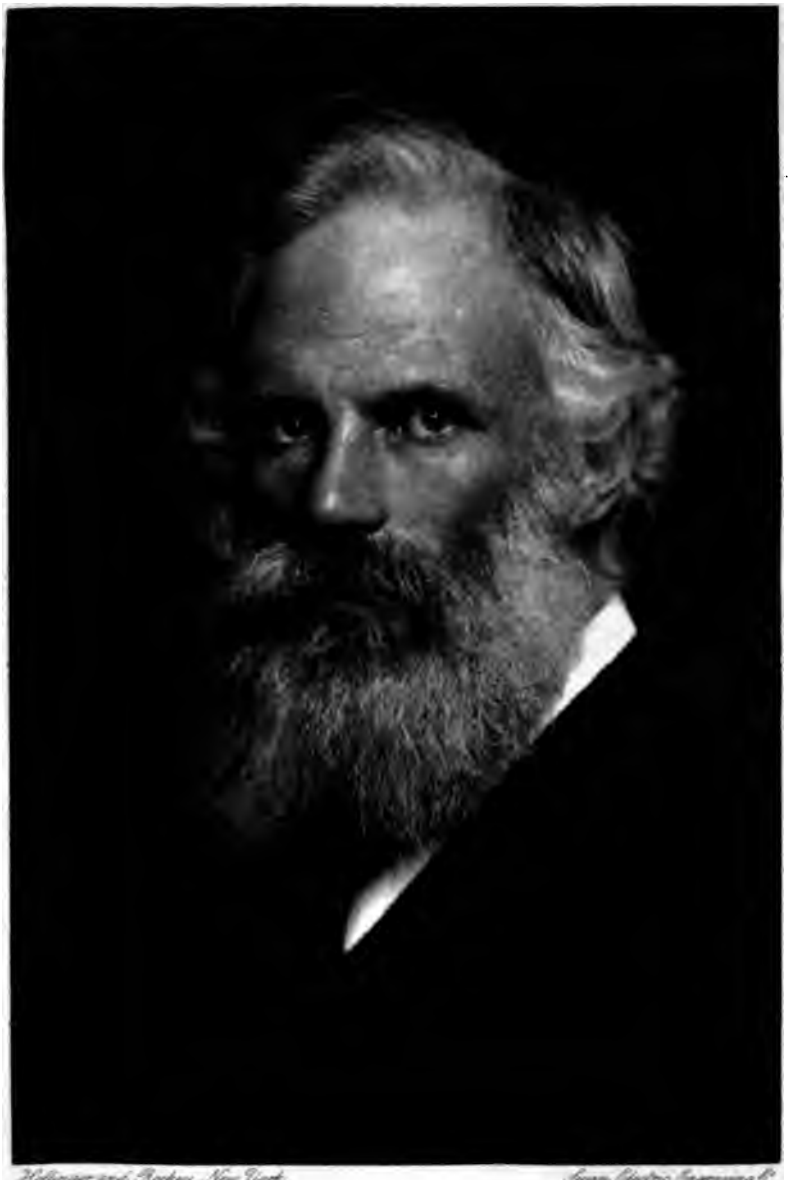

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COLLECTED AND EDITED WITH A HISTORICAL INTRODUCTION

BY

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FELLOW OF ST JOHN'S COLLEGE CAMBRIDGE

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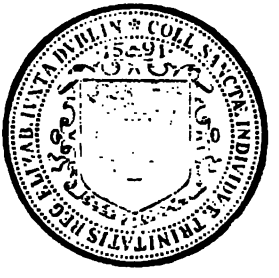

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PREFACE

When Professor FitzGerald's career came to a premature and lamented close, at the beginning of last year, it was widely felt that his most suitable and enduring scientific memorial would be a collected edition of his published work. The present volume is intended to fulfil that object. An endeavour has been made to include everything substantial of a scientific character that he wrote; and the omissions, so far as is known, either relate to discussions of only temporary interest, or are re-statements of work included in other forms.

By Professor FitzGerald's scientific friends, who may be taken to include nearly all persons interested in the higher progress of physical science, the book will be welcomed as a reminder of his incisive thought and his manifold and helpful scientific activities. But it is anticipated that it will also serve another purpose. The progress of physical science during the last forty years has extended our ideas of the constitution of inanimate nature in a way which has been one of the marvels of the age. When the subject is trimmed down to the precision of a

formal text-book, much of its charm and instruction is thereby evaporated. We need to live with the men who have been instrumental in producing the change of view, to observe intimately their habits of thought, their failures as well as their successes—to take note of the hitches in the path of progress, some of them gradually removed by the acquisition of clearer or broader ideas, others as yet defying complete resolution—before we can assimilate all the intellectual advantage that is to be gained from a study of scientific evolution. The republication in accessible form of the work of Helmholtz, Hertz, Kelvin, Maxwell, Rayleigh, Stokes, and other masters, has thus done much to maintain the vitality of theoretical physics, and constitutes a precedent which ought to be preserved.

The very brevity of most of FITZGERALD'S papers, and their direct attack on the barriers that limited the scientific horizon of the day, enhance their value towards a historical conspectus of the progress of the theory of the ether, and of the branches of physics which are founded thereon. That the reader may have an indication how long ago ideas were already in the air in this department of knowledge, that have only recently been emerging into complete precision, the year of original publication has been inserted on the headline of each page. It has been well remarked that work by a master gains rather than loses in intrinsic interest, by the establishment of a point of view and a range of ideas wider or more definite than those under which it was produced.

As the gist of these papers lies mainly in the train of ideas, and but slightly in the algebraic or numerical calculations which occur in them, misprints and obvious mistakes in the latter have been corrected where necessary, without special mention. This course has been rendered safer by the vigilance of Professor W. E. THRIFT, F.T.C.D., who has supplied a number of such corrections, and has also verified others.

The notes in square brackets [. . .] have been added by the Editor; they are intended to draw attention to discrepancies in the main argument, or divergences from results more recently established, as well as, in some cases, to indicate the recent history of the problems that are treated in the text.

The task of sorting out the papers and arranging them for press has been much facilitated by the kind assistance of Mrs. FITZ GERALD. Valuable advice and information on personal matters have also been throughout freely given by Professor MAURICE FITZ-GERALD and Mr. E. P. CULVERWELL, F.T.C.D.

Assistance has been afforded by Professor J. JOLY, F.R.S., Professor F. T. TROUTON, F.R.S., and Mr. W. E. WILSON, F.R.S., specially in connexion with papers of which they are joint authors, which have been included in the collection.

Thanks are due to the Royal Society, the Royal Dublin Society, the British Association, the proprietors of the *Philosophical Magazine*, of *Nature*, and of the *Electrician*, for permission to republish papers from their collections, and for the loan of blocks for the illustrations.

The beautiful portrait frontispiece was originally intended to have been a contribution to the volume from some of Professor FITZGERALD's friends in England, but Mr. J. W. SWAN, F.R.S., has very kindly insisted on bearing the whole of the expense connected with it.

Acknowledgment is due to the Board of Trinity College, Dublin, for the readiness with which they undertook the financial responsibility for the volume, and to the officials of the Dublin University Press for the careful manner in which they have carried out its preparation.

CAMBRIDGE,
May, 1902.

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ADDENDA AND *CORRIGENDA*

p. 42, first formula, for γ read y.

p. 66, see footnote, p. 162.

p. 80, add reference *Roy. Soc. Proc.*, 63, 1898, p. 371.

p. 207, last line, insert e.

p. 412, first §, this effect is of the second order at most, not the first as stated Cf. No. 108.

p. 468, at end. Kayser and Runge give for air (Akad. Berlin 1893),

$$\mu = 1{\cdot}00028817 + 1{\cdot}316 \times 10^{-14}\lambda^{-2} + 3{\cdot}16 \times 10^{-24}\lambda^{-4},$$

which would make λ_0 equal to about 50×10^{-8}, so that the single absorption band postulated would be very far up in the ultra-violet.

INTRODUCTORY AND BIOGRAPHICAL

[From the *Electrician*, March 1, 1901. By Principal O. J. Lodge, F.R.S.]

At the same age as Clerk Maxwell (*ætat.* 49), Fitz Gerald has gone from us: there has been no equal blow to Physical Science between the two events, in this country, possibly not in the world: for the untimely death of Hertz himself was no greater blow. Old men pass in the course of nature, and we mourn them; but the departure of a bright genius full of power and originality, trained by years of effort and self-denial to do work which no one else can do, strikes one with a chill feeling of calamity.

Why do they die so young? They are overworked—undoubtedly they are overworked. The problems of highest physics are too appallingly difficult; ordinary humanity knows nothing of them; it does not realize the burden of thought which weighs on the shoulders of the few who can solve them: it cannot realize the mental strain, the continued weeks of thought which must go to the highest achievements in physics. Such men should live apart and be preserved from the every day struggle of common life; but that is not our system. Nature is prodigal, and we imitate her wastefulness in dealing with genius. Let any Dublin man bear testimony: let us be told how many committees and boards and meetings Fitz Gerald attended; let us hear how constantly he was used by societies as scientific referee, how many proof-sheets of others he had to read when they were writing books, how he was appealed to in all cases of difficulty, not only by his colleagues and disciples and pupils in Ireland, but by the whole physical and chemical English speaking world.

Was it a waste of time, this help freely given to others, this self-forgetful energy in the public service? Who is to say? It was waste of health, but it stored up in the minds of many a mighty affection for their helper. No, it was not waste of time.

A good man he was if ever there was one: devoting himself to public work, free from all petty ambition, caring nothing for himself: throwing out his ideas right and left with splendid prodigality, rejoiced if they were absorbed and utilized by others. How quick he was to seize a point, and how keen to criticise, to extend, to illuminate, a subject. Put near his elbow an abstruse Paper, he would take it up casually, as another might a newspaper, and soon he would be absorbed and begin pencilling out calculations indicative of luminous ideas.

See him at a British Association meeting of Section A, he was the life and soul of debate; he was always ready with some semiparadoxical but wholly suggestive and stimulating idea. See him at that memorable meeting at Bath, when he brought forward and made known to the world the brilliant experimental discoveries of Hertz, discoveries which verified Fitz Gerald's own suggestions and surmises. Hear him at that same meeting when he was discussing with Lord Kelvin and others about Ψ and J and the propagation of electrostatic potential in time, and the other outstanding difficulties in Maxwell's splendid work. Turn back to the *Electrician* for 1888, vol. xxi., pp. 624, 660, and read it again.

There did not exist a brain quicker to seize a point, fuller of ripe learning, readier to apply it in any direction, less absorbed in itself, and more willing to enter into the work and the difficulties of others; nor was any mind keener to apprehend rightly the real difficulties of Physics, or more fruitful of verifying suggestions. Let Larmor tell us of his help in Mathematics, let Ramsay tell us of his help in Chemistry, let Perry tell us of his help in problems applied to Engineering, but let me bear testimony to his simple, human, unselfish, public-spirited, hard working college and family life. Ay, he was a man worth knowing. His memory will not fade. Long life could not have bettered it. He has lived his life, and has left an influence on his generation as potent as his influence on the progress of physical science.

It has been a great epoch this latter half of the nineteenth century, a period with which his life was conterminous; but it seems closing in. Yet no, there are a few men left, and discoveries are in the air. We must close up the ranks and proceed, thankful that we have known him, and not disheartened or dismayed.

His powers blossomed early, and he had a good education. He did not go to school, but absorbed mathematics at home under tutors who must have known and loved their subject; and from them he did not

learn the inattention and listlessness which seem inseparable from class work under average teachers.

He possessed extraordinary versatility, and could turn his mind almost instantly to anything, but the instant it was so turned it went deep into the subject, to the exclusion of other things for the moment; and in the deepest subjects he was more at home than in the trivial and superficial. But he was never a recluse; had he been more of a recluse perhaps his great power of intimate brooding and absorption, combined with his wide mathematical knowledge and preparedness, might have led him to some epoch-making discovery.

But if so he did not give himself the chance; his place was with the captains and the shouting, and the intervals of leisure for real continuous work were few and far between: hence he leaves no book and few papers. Not so few as people think, however; many were published by the Royal Dublin Society, and these may be collected and made more accessible in future; but very few indeed compared with his powers and the fertility of his brain.

Had he lived on into the more serene atmosphere of a Senior Fellowship of Trinity College, Dublin, he might have put all his work together and brought out a suitable treatise which would serve as a memorial, a landmark for later time; but he lived a hard and strenuous life, and the era of peace was still in the future when he died.

I saw much of him at Bradford last September, and he spoke of indigestion and of needful care and caution with food; it did not seem to me serious, but a few weeks later he wrote to say that problems stayed on his mind more than they used, and interfered with him and could not be shaken off, and that he was not allowed to think continuously. He feared that he might be permanently debarred from thinking if he were not careful. The letter frightened me, and I wrote round to several who were likely to correspond with him, and to some in Dublin, warning them of danger and begging them to be careful.

Then the indigestion grew worse, and he had to have complete rest and isolation from letters; but the internal trouble continued, and suddenly intensified; then an operation, and the peaceful rapid end.

The electricians of the world will not resent these details: surely they regarded him as a friend. They will wish to know the cause and the manner of the end. Into many parts of the Earth, wherever cables

are laid, and wherever man talks Morse, the death of Fitz Gerald has already gone: it is fitting that these unknown friends of his should hear the few details which we in England know.

It is right that such a man should be honoured for his great learning, high powers, and bright achievements, on which many from various points of view can speak; but it is right also that he should be be lamented on his human side by one who loved him as a brother.

(From *Nature*, March 7th, 1901.)

Those who knew the University of Dublin twenty years ago will remember that the idol of the undergraduates and the hope of the older men was George Francis Fitz Gerald. He was of high intellectual lineage on both sides: his father was the most distinguished prelate in the Irish Protestant Church, and his uncles are men of large and original scientific achievement. His early education was conducted at home, in company with his two brothers, one (now professor of engineering at Belfast) a year older than himself, the other younger. He was good at physical science and all subjects requiring close observation, from his earliest years; and the ambition to become a master was soon aroused. The mathematical and physical tendency seems to have come mainly from his mother's side, his strong metaphysical bent from both sides of the family. In his student career he attained all the distinctions that lay in his path with an ease, and wore them with a grace, that endeared him to his rivals and contemporaries. On taking his first degree in 1871 he settled down, at twenty years of age, after the manner of the pick of the Dublin men, to a wide and independent course of reading with a view to a Fellowship. At that time vacancies were of very rare occurrence; so that it was not until 1877, on his second time of trying, that he attained the position of a Fellow of Trinity College. The examination in mathematical and physical science included papers on selected portions of the works of the great mathematical physicists; to a mind of the calibre of Fitz Gerald's, the early and intimate acquaintance which was thus promoted with the classical writings of Lagrange and Laplace, of Hamilton and Mac Cullagh, with their modes of thought as well as the results that they won, must have formed the best possible foundation for a scientific career. A training which aims

only at sound knowledge and established results may find a shorter path in the study of the latest text-books of the day; but if a man is to be a true leader he must be interested even more in the philosophy than in the facts of his science. It must have been of rare value to a maturing mind of keen temper to observe closely at first hand the lines of attack of the great masters of the past age on problems which were crystallizing into knowledge. Acquaintance with the present state of science, however detailed and exact, assumes its full value as an instrument of progress only when it is accompanied by appreciation of the difficulties that had to be circumvented in order to reach it, and by observation of the way in which complete logical precision may have to be attained at the expense of temporary limitation. The subjects that were grouped around physical optics were approached in Dublin, in those days, through the study of Mac Cullagh's optical memoirs; these writings were based on a remarkable combination of keen analysis of the facts and direct application of the generalised dynamical methods of Lagrange, thus presenting all that interest of nascent scientific discovery which the same topics still retain in their wider connexion with the general problem of the ether. Whatever may be the defects of Mac Cullagh's analysis, it had the saving merit that it put forward no claim to finality; its critical comparison and contrast with those of Cauchy and Neumann and Green, and the difficulties which its procedure suggested from a restricted dynamical point of view, were the very things with which a mathematical analyst might be impatient, but over which a mind constituted like Fitz Gerald's would eagerly brood. When the great Treatise of Maxwell, which threw a flood of light on these fundamental problems from an altogether novel source, came into hands thus prepared for its appreciation, it is not surprising that a main scientific interest became established for life.

After obtaining his Fellowship, Fitz Gerald became attached to the department of experimental physics, and conducted or influenced much of the teaching in physical science, in addition to carrying on the work of a College tutor. In the latter capacity he was eminently successful. It was an object of ambition to gain admission to his side, which was always full a long time in advance. He had considerable athletic prowess, which was kept up for many years; and his services were in great request for presiding over and administering the athletic organisations of the College. He gave up tutorial work in 1881 on

succeeding to the Chair of Experimental Philosophy, which he held for the rest of his life. He became a Fellow of the Royal Society in 1883, and in 1899 received the award of one of its Royal Medals.

In those early years there were three main centres of development of the new departure in electrical theory which has since revolutionised the whole domain of physical science. Maxwell's own presence as a professor had guided the trend of physical thought at Cambridge predominantly into that direction which it has since largely retained; in Berlin, Helmholtz was devoting his great powers and turning the attention of his pupils to the discussion and elucidation of the subject; while in Dublin its study and investigation became vital under Fitz Gerald's lead and influence. His chief formal memoir, "On the Electromagnetic Theory of the Reflexion and Refraction of Light," was presented to the Royal Society at the end of the year 1878; it retains a place among the classical writings of modern physics. In the years from 1880 to 1885 he contributed to the publications of the Royal Dublin Society, of which he was secretary for about ten years, many short condensed papers, on the optical and electric influence of the Earth's motion, on the amount of the electric radiation from an alternating current, on a model illustrating the properties of the ether, all of which went straight without superfluous analysis to the core of the matter on hand, and eminently merit Mr. Heaviside's description (*infra*) as "not large in bulk, but very choice and original." At the present time, after so much progress has been made in the abstruse but fundamental topics with which they deal, these pioneering papers, like Maxwell's Treatise, on which they are based, still repay careful study. It is much to be desired that they may soon be republished in more accessible form, along with their author's other scattered writings.

As years passed, the calls on his time became more numerous, in the tender care of his family, in the discharge of public duties, and in response to requests for advice from an ever-widening circle of devoted scientific friends; so that his opportunities for continuous study almost disappeared. But he always managed to keep wonderfully abreast of scientific progress in a very wide range of knowledge, and spent most of his spare time in deeply pondering over its meaning. The scientific public of this country was placed very early in touch with Hertz's magnificent and decisive verification of electrodynamic theory, through the attention commanded by Fitz Gerald's brilliant exposition in his

British Association address of 1888. It was fitting that this should come from him; for, as Lord Kelvin has recalled, he had five years before pointed out to the British Association the possibilities of the very plan of obtaining electric radiation of manageable wave-length which in Hertz's hands has led to success. His own activity became more and more absorbed in the administration of the College laboratory, rendered more arduous by limited funds and distance from other scientific centres, and in the promotion of the practical and technical side of physical science. Yet he still followed very closely the progress of abstract mathematical physics; hardly anyone could be named who had thought more deeply, or whose knowledge was more available and many-sided, more entirely free from all prepossession or prejudice. At the meeting of the British Association last September he was, as usual, present, and was of course one of the prominent figures; the writer, speaking from full knowledge, can testify that the proceedings of the physical section were interesting and succesful from one cause beyond all others—the assiduity with which he devoted himself to attendance, and the unceasing flow of valuable suggestion and appreciative criticism which he contributed. His stores of knowledge were ripening and maturing in fibre year by year; his memory was unfailing, and each new fact or phenomenon seemed to find its place at once in the setting to which it belonged. Whatever views were presented to him, however much they jarred with his own ideas, were certain to receive patient and careful consideration. There was nobody who did more to encourage younger men and to bring out what was best in them; the time which he was accustomed to devote without stint to the elucidation and improvement of the work of others sadly diminished the opportunities for work more especially his own. His advice and judgment were valued over the whole range of physical science, not less in foreign lands than at home, notwithstanding that he published so little. When a physicist or physical chemist came to a puzzle or paradox, or was in doubt between various plans of procedure, it seems to have come to be almost the natural course to write to Fitz Gerald. A letter of inquiry or criticism always elicited a prompt reply, entirely devoid of pretension to magisterial authority, but certain to bring out new aspects of the subject and exhibit its connections with other problems. He was constantly acting as referee of scientific papers for the Royal Society and other bodies, and was accustomed to interest himself in them as if they were his own work.

He frequently acceded to requests to serve as examiner on physics in other Universities, notwithstanding the serious drafts on his time and energy that were involved; his connexion with the University of London in that capacity has been almost uninterrupted since 1888. He became one of the Commissioners of National Education in Ireland in 1898, and immediately threw himself into the task of reconstituting primary education on more practical lines, undertaking a tour through the United States in the autumn of that year in order to study American methods. Last year he was appointed a member of the Irish Board of Intermediate Education, and much was expected from his assistance in working out the difficult problems that engage their attention. In his own University he was always in the forefront of progress, and often wished to move faster than an ancient institution is usually inclined to allow.

In a private letter, in response to a hurried intimation of Fitz-Gerald's death, Mr. O. Heaviside writes as follows:—"I only saw him twice knowingly, once for two hours, and then again for six hours, after a long interval; yet we had a good deal of correspondence at one time, and I seemed to have quite an affection for him. A mutual understanding had something to do with that. You know that, in the pre-Hertzian days, he had done a good deal of work, not large in bulk but very choice and original, in relation to the possibilities of Maxwell's theory, then considerably undeveloped and little understood; and his way of looking at things was more like my own than anybody's. Well, he found that I had done a lot of work in the same line, and he was most generous in recognising and emphasising it. Too generous, of course. You remember that review of my 'Electrical Papers' that he wrote? No one knew better than myself how to allow for his temperament and desire to help me. He used to write to me a good deal about electromagnetic problems, and I laid down the law to him like—like myself, in fact. He took it all very pleasantly. But I knew all the while that he had a wider field than myself, and no time to specialise much. He had, undoubtedly, the quickest and most original brain of anybody. That was a great distinction; but it was, I think, a misfortune as regards his scientific fame. He saw too many openings. His brain was too fertile and inventive. I think it would have been better for him if he had been a little stupid—I mean not so quick and versatile, but more plodding. He would have been better appreciated, save by a few."

Prof. W. Ramsay writes on the day following his return from India, when the first news came to him:—

"I understand that it has been thought right for some of Fitz-Gerald's friends to contribute each a short notice of him as a tribute to his memory. The blow is so recent and the feeling of personal loss so acute that this is a difficult task. But to me, as to many others, Fitz Gerald was the truest of true friends; always interested, always sympathetic, always encouraging, whether the matter discussed was a personal one, or one connected with science or with education. And yet I doubt if it were these qualities alone which made his presence so attractive and so inspiring. I think it was the feeling that one was able to converse on equal terms with a man who was so much above the level of one's self, not merely in intellectual qualities of mind, but in every respect. I know that Fitz Gerald would have been the last to acknowledge this, for he had no trace of intellectual pride; he never put himself forward, and had no desire for fame; he was content to do his duty. And he took this to be the task of helping others to do theirs. This was happily expressed by the President of the Royal Society in awarding him one of the Royal Medals, when he alluded to the great influence exercised on the progress of science, due to Fitz Gerald's placing his services unreservedly at the disposal of everyone anxious to carry on physical or chemical research.

"I do not think that Fitz Gerald ever harboured an angry or uncharitable thought about anyone, nor have I ever known anyone who, knowing him, did not regard him with the greatest love and respect; for he was known to be absolutely true to himself, and therefore to his fellow-men. Although he held strong views on many points, and could defend them with vehemence, his argument was never a personal one; and it was obvious that he was actuated solely by a love of truth, and that his only object was to defend what he thought to be right. Moreover, what Fitz Gerald thought to be right was pretty sure to turn out to be right in the long run. May I suggest as the reason why Fitz Gerald was so universally beloved, that he was a Christian in the truest sense of the word, and that he followed very closely the footsteps of his Master?"

In a letter from Prof. Tilden to the *Times* of February 27, in which, as Dean of the Faculty of Science of the University of London, he "places on record their high appreciation of his brilliant qualities as a man, as a teacher, as an investigator, as a leader of scientific thought,"

he goes on to speak on his own behalf of Fitz Gerald's modesty and extreme unselfishness, of the clarification and enlightenment which many a scientific man has owed to his inspiring conversation, of the "most memorable discourse" which he delivered to the Chemical Society in 1896 as the Helmholtz lecture. The key of deep personal loss is struck in a touching and eloquent tribute communicated to the *Electrician* by Dr. O. Lodge, and in a shorter notice in the *Times* and the *Philosophical Magazine*, coming from one who writes with authority on the industrial applications of science.

Prof. Perry, speaking as President of the Institution of Electrical Engineers, records that "in all engineering questions he had not only the laboratory experimenter's point of view, but also that of the practical engineer. His was a mind that saw the bearing of all scientific knowledge on any practical problem. I have no hesitation in saying that in Prof. Fitz Gerald our profession has lost one of its greatest, most beneficent forces." In proposing a vote of condolence, Prof. Ayrton spoke to the same effect.

The pride and affection which he inspired in his own College is revealed in a masterly appreciation contributed to the *Athenæum* by one of his colleagues on the literary side. "His appearance was not unworthy of his fame. More striking he was than handsome; but his ample grey locks and beard, his furrowed brow, his penetrating eyes, reminded one of the bust of some Greek philosopher, which we cannot look upon without that instinctive feeling of respect which intellect and character command among civilised men."

For some years he had been in precarious health. He was subject to recurring attacks of digestive trouble; but the buoyancy with which he threw them off, and the unabated zeal with which he returned to his scientific pursuits in the intervals of health, concealed the real gravity of the situation. News of a sudden crisis was received in London and Cambridge with universal feelings of deep concern. He has now passed (on February 21) from the scene of an active and most beneficent career, in the fiftieth year of his age. His memory will not die. It will be carried on by a school of experimental physics, including the names of Joly and Preston and Trouton and W. E. Wilson, which he was mainly instrumental in creating; while in a wider sphere men such as Heaviside and Lodge and Ramsay and Perry have been proud to testify to their indebtness and to claim him as their master. His scientific place will he henceforth alongside Rowan

Hamilton and MacCullagh and Humphrey Lloyd, and the other famous men who have secured for the Dublin school so prominent a position in the edifice of modern physical science. In the higher domain of heart and conduct the recollection of his qualities will be an abiding treasure to all who knew him.

J. L.

[From the *Proceedings of the Institution of Electrical Engineers*, 1901. By Prof. F. T. Trouton, F.R.S.].

George Francis Fitz Gerald was born in 1851 at the old Rectory, Kill-o'-the Grange, Monkstown, Co. Dublin, of which parish his father, afterwards successively Bishop of Cork and Killaloe, was then rector. His education was a home one under tutor, until he entered Trinity College, Dublin, at the age of sixteen in 1867. His Undergraduate Course in the University was a most distinguished one, he taking the highest place in Mathematics and Experimental Science at his Degree in 1871. He gained a Fellowship in 1877, and in 1881 was appointed the Erasmus Smith Professor of Natural Philosophy, which post he held for the rest of his life. He was elected a Fellow of the Royal Society in 1883, and in 1899 he had conferred upon him one of the Society's Royal Medals. He was made an Honorary Fellow of the Royal Society, Edinburgh, in 1900.

Up to his appointment to the Professorship there had been no teaching in Dublin in Practical Physics, but on his obtaining possession of an old disused chemical laboratory, he started classes in Experimental Physics, and shortly gathered round him earnest workers in that branch of science. He ever had at heart the claims of experimental science in his University, and though he succeeded in effecting considerable advances, it remained to the end a source of disappointment to him that he could not move the authorities to bring things up to the requirements of the day.

Fitz Gerald was early impressed in electrical matters with Maxwell's views, then new and imperfectly understood by the scientific world, and actively introduced them into his teaching. He published in 1876 his first paper on the "Rotation of the Plane of Polarisation of Light by Reflection from the Pole of a Magnet," and in 1880 his paper on the "Electro-magnetic Theory of Reflection and Refraction of Light," in which he showed how the relations established in the older theories were in agreement with Maxwell's "Electro-magnetic

Theory of Light" and afforded it signal support. From that time he continuously interested himself in this subject, and in the possibility of obtaining by electromagnetic means Maxwellian Radiation.

Many of his papers to the Royal Dublin Society and elsewhere elucidated this question, and he succeeded in devising means, the only one still available, for producing these electromagnetic waves. He was deterred from making practical application of them by not seeing at the time how the presence of these waves might be detected. He thus was prepared for and warmly welcomed the announcement of Hertz's achievements in electromagnetic radiation, and brought these discoveries prominently before the English scientific world in his address to the Mathematical and Physical Section of the British Association in Bath in 1888, and later in a lecture at the Royal Institution. Both these discourses were rich in pregnant ideas and suggestions, which helped on and inspired in no small degree the great body of work which was shortly to put our knowledge of electromagnetic radiations into the position of being available for practical applications.

In 1885 he invented and constructed a model illustrating Maxwell's theory of electric and magnetic displacement, which was of considerable influence in meeting the objections then often urged that the ether postulated was unmechanical.

He published in 1879 a paper on "Vapour Pressure near Curved Liquid Surfaces," in which he gave an explanation based on molecular considerations of the well-known relations established earlier by Lord Kelvin from thermodynamics. He contributed important papers on numerous other questions to various scientific societies which are to be found in their publications.

His mind was of an eminently practical type and, had his life been one thrown more in immediate contact with the pressing problems of engineering, would doubtless have produced much fruit in inventive applications of science.

For many years he took an active part in general educational matters, both in his own University and elsewhere. To his assistance was largely due the inauguration in Ireland of technical education, and the founding in Dublin in 1887 of the Kevin-street Technical Schools, in which he continued until his death to take an active interest.

He was a Member of the Board of National Education for Ireland,

and to him in great measure must be attributed the improvements of late introduced into the system of primary education. He was a member of the recently created Board of Technical Instruction for Ireland, to which he was able to give, on its inception, important guidance through his extensive knowledge of the subject. He was also a member of the Board of Intermediate Education.

His friends felt for some time that all this work for the public weal, when added to his scientific and professional duties, was growing beyond his powers of physical endurance. It no doubt tended to aggravate during the last year the digestive disorders he was subject to, and to which he succumbed at the comparatively early age of forty-nine. He passed away at a time when his great knowledge and matured judgment were being fully appreciated, and when his attainments were being freely utilised for the benefit of his country.

Fitz Gerald was himself a most enthusiastic teacher, and his pupils ever felt, even in the most commonplace subjects, that they were being carried along with him in paths leading to new realms of knowledge.

His judgment and advice on matters relating to scientific research were widely appreciated. This entailed an ever-increasing correspondence which he ungrudgingly maintained, giving help to other workers in science the results of which it would be hard to over-estimate.

His acquaintance with what had been already done in science was extensive. This, with his own inventive skill, was always at the disposal of those seeking his assistance.

At scientific gatherings and discussions he stood out pre-eminent through a power of rapidly grasping the essential, and few subjects arose on such occasions to which he did not add interest from his clear penetrating vision.

His presence was always a welcome one among his friends, and ever carried with it a feeling and influence of an elevating nature. It is given to few men to leave with their very intimates a memory, as it were, of a superior being; such did he.

Professor Fitz Gerald was President of the Physical Society of London in 1892. He was elected a Member of the Institution of Electrical Engineers on the 10th of December, 1891. On the formation of the Dublin Local Section in 1899 he was elected first Chairman of the Section, and his inaugural address in that capacity was printed in the Journal of the Institution in 1900.

[From the *Obituary Notices of the Royal Society*, 1901.
By Principal O. J. Lodge, F.R.S.].

A THOROUGH attempt to estimate the scientific value of Fitz Gerald's life and work cannot yet be made: a summary of his published writings can be given, and an indication can be added of the high estimation in which he was held by scientific men in these islands. To the foreigners and to men who have not been brought into immediate contact with him his reputation may seem hardly intelligible; and, indeed, we are often constrained to plead guilty to a sort of family affection existing among British Physicists, and a sympathetic understanding running through our appreciation of them, which tempts us occasionally to be unduly inattentive to some of the first-class work of Physicists outside. It is not a fault on which we pride ourselves: it is one which we lament: it is one which may shortly cure itself, as death removes one after another of those countrymen of the last generation whom we have held in such high honour. The time will doubtless come when, with our eyes opened by bereavement, we can estimate in a manner less hampered by intimate insular knowledge the equal achievements of foreigners; but meanwhile we must plead as excuse the extraordinary personal merit and character of some recent Physicists, notably of Fitz Gerald, in whom those who knew him best could detect least flaw. But foreigners came under the same influence when they were brought into contact with him: witness Ostwald at Leeds, and Lenard at Liverpool; it may be doubted whether there ever was a man of equal scientific power, agility of thought, and selflessness, combined.

George Francis Fitz Gerald was born on August 3, 1851, and died on February 22, 1901, before he was fifty years of age.

He was the second son of the Rev. William Fitz Gerald, D.D., afterwards Lord Bishop of Cork, and later of Killaloe: being at the time Rector of St. Ann's, Dublin. His mother was a sister of Dr. G. Johnstone Stoney, F.R.S. He did not go to school, but was educated at home under stimulating circumstances, and to this fact may be attributed some of the retention of his innate originality.

He was not specially remarkable for early ability, as he did not possess any conspicuous faculty for acquiring languages or other learning involving verbal memory; he was good at arithmetic, algebra, and Euclid, of an inventive turn of mind mechanically, and

skilful with his fingers in sewing, knitting, and such work; also he developed considerable athletic powers, though he was never specially competent at games.

While still only 16 he went to Trinity College, Dublin, where he soon took a high place; and on obtaining his degree, in 1871, he won the University Studentship, with two First Senior Moderatorships in Mathematics and Experimental Science.

During six years of post-graduate study for the Fellowship, he laid the foundation of his deep and wide knowledge of Physical Science by study of the classical writings of some of the Masters in Mathematics and Physics, notably Lagrange, Laplace, Hamilton, and Mac Cullagh. He also made some study of Metaphysics, and was permanently attracted by the philosophy of Bishop Berkeley.

In 1881 he was elected to the Erasmus Smith Professorship of Natural and Experimental Philosophy, vacant by the death of Prof. Leslie; and work gradually began to accumulate upon him. Fortunately he was able to turn his mind readily and persistently to anything that was brought before him, and in the midst of interruption could sit absorbed in either reading or jotting down calculations, sometimes of considerable complexity. But the leisure for long patient analysis was not his, nor did his genius altogether lie in this direction: he was at his best when, under the stimulus of discussion, his mind teemed with brilliant suggestions, some of which he at once proceeded to test by rough quantitative calculation, for which he was an adept in discerning the necessary data. The power of grasping instantly all the bearings of a difficult problem was his to an extraordinary degree, and it was rare indeed that a thought or a difficulty could be presented to him over which he had not at some period previously brooded. If it were not so—if the ideas were really then presented for the first time—his quickness in seizing them was miraculous. It is easier to suppose that during his long and strenuous course of reading, and in the stimulating mental atmosphere of Trinity College, in conversation also with his uncle, Dr. Johnstone Stoney, and others, nearly all the problems in physics likely to occur to contemporaries had, in some form or other, come within his ken: and hence hardly anything that could be suggested seemed altogether new and strange to him. Nor did his knowledge seem to have sunk into any kind of oblivion; there it was, always accessible, and with an added commentary of his own quite

ready, to the surprise and delight of those who conversed with him.

So, for instance, occurred his perception of the influence of light-pressure in Astronomy; also of the emission by the Sun of electrified particles which streaming past the earth might give rise to magnetic storms and auroræ, before our knowledge of electrons made this idea easy or quantitatively feasible. So also occurred that brilliant suggestion of the change of shape or distortion due to motion through ether, now known as the Fitz Gerald-Lorentz hypothesis, which flashed on him in the writer's study at Liverpool as he was discussing the meaning of the Michelson-Morley experiment. Of this nature also was his suggestion to utilise the oscillatory discharge of a Leyden jar as a means of exciting ether waves: an idea which roughly had occurred to others before (the writer finds it in one of his own note-books of date 1879–80), but with Fitz Gerald it became quickly definite, leading him to investigate not merely the easy problem of the wave-length to be expected, but the much more difficult question of the amount of power that would be radiated by an alternating current in any given case.

Directly Hertz's experiments were published, Fitz Gerald discerned their whole significance, and in his brilliant Presidential address to Section A of the British Association, at Bath, called the world's attention to them in an unmistakeable manner. Had it not been for the English recognition they received, it is improbable that the work of Hertz would have been hailed with the immediate chorus of universal approbation which it commanded; for the work of his own countrymen had mainly lain on other lines; and even to Hertz himself the theory of Clerk Maxwell only gradually, and subsequently to his verifying experiments, became quite clear and familiar. Undoubtedly Fitz Gerald recognized more vividly than Hertz himself at that time the full import of his experiments,—the German title of which was far from representing the plain significance of the title applied by Lord Kelvin to the later English translation, viz., "Electric Waves." This is no disparagement to Hertz: rather it strengthens our admiration of him to perceive how quickly and perfectly he could emancipate himself from national traditions, and constitute himself an apostle, and one of the most powerful exponents, of the Maxwellian Theory of Light.

But to Fitz Gerald all this was fundamental and familiar—he had

got beyond the analysis, and revelled in full-bodied conception and pictorial imagery and mechanical models of what was going on; and these clear perceptions of his, with a realization of much of this outcome that might be expected, were really of more value and contributed more to the progress of science than did his own laborious analytical investigation of the Electro-magnetic Theory of the Reflexion of Light from Insulators, from Crystalline Bodies, and from Magnetized Media, which constitutes his chief systematic memoir: powerful and impressive as the complete mathematical analysis of so difficult a subject necessarily was.

Another aspect of the man was his extraordinary and sympathetic critical power. He did not seem to mind reading other people's papers and proofs; entering into their point of view seemed to him to present no difficulty, nor did the immediate correction of blunders into which they might have fallen seem to present any difficulty, or suggest any claim to superiority.

As an ordinary man can correct a schoolboy's sum, or an exercise in simple mechanics or geometry, so he could tackle a difficult Royal Society paper, or a Treatise say on Thermodynamics or on Physical Chemistry, and point out both the merits and the flaws in it at once. Never was anyone so clear on the subject of the pitfalls which once awaited the unwary chemist or applier of the second law of Thermodynamics to Physical and Chemical problems: the result being correct, or at least acceptable, it is so easy to bolster it up by a false application of quasi-mathematic or thermodynamic reasoning; but all such fallacies were instantly detected by Fitz Gerald, and the essential requirements of both reversible and cyclical processes, as the basis of systematic theory, insisted on.

Suggestions for experiment frequently occurred to him, but were seldom carried out with his own apparatus; rather he preferred to hand on both the labour and the honour of an experimental research to some assistant or student, to whose reputation a successful result would make all the difference; and many results obtained by others probably owe their initiation to him.

By reason of these peculiarities of disposition his published memoirs may not impress foreigners, or those who did not know him, with a proper idea of his real magnitude; but it is probable that they will nevertheless produce a very considerable impression. For so many of his contributions to Science were made to the Royal Dublin Society (of

which he acted as Secretary from 1881 to 1889), or orally to the British Association — neither of which agencies is specially well adapted for informing the world generally—that to many the memoirs which will shortly be published, under the careful editorship of Dr. Larmor, will be new, and will come as a revelation of solid and industrious work.

Nevertheless, once more it must be said that his wide knowledge, and brilliant speculations based upon that knowledge, were what impressed his friends the most. Sometimes they seemed almost too fanciful, too far-reaching ahead of solid fact, too intangible and fantastic to be attractive; that is the case to some extent, for instance, with parts of the Helmholtz Lecture, where the beauty and the possibilities of the vortex hypothesis of the constitution of matter and of the structure of the ether entice him into regions where substantial mathematical progress is hardly yet possible. Into this region, however, the human race must advance, if it is to proceed with the unification of matter and the more fundamental understanding of the material universe; and our descendants—the possessors of an elaborated theory—will be able to judge better than we can how far these speculations of FitzGerald were fantastic imagination, and how far they were the outcome of a real and semi-inspired insight into the inmost processes of Nature.

But in spite of his ready absorption in these physical topics and his almost unique power of quickly grasping and fruitfully dealing with them, he was imbued with a sense of the far greater importance of humanity itself, than of any of these material things. In fact it was this constant feeling of the value of human relationships, and the supreme influence of good feeling and affection, that led him to regard all questions of priority or of scientific credit with not so much disdain as absence of interest. It is easy to say that provided a discovery is made it matters little who makes it, but it is not so easy constantly and consistently to feel and act in that spirit; but so far as it can be done FitzGerald did it, and did it apparently almost without an effort. The things he really valued were the things belonging to the human spirit—the development of the individual and the development of the race. Anything which hindered this met with his strenuous opposition: self-satisfied unprogressiveness in educational matters excited his wrathful and outspoken indignation; and on these subjects alone did he occasionally make enemies. Other

things might be of intense interest but were not of supreme value, and to sacrifice any personal relationship to them was worse than useless.

With all his critical power he seldom expressed himself severely on the scientific mistakes of others. I have once or twice heard him speak of some man as small or narrow, and I have heard him wax indignant over some charlatan who pretended to be what he was not; but these were exceptional instances, and as a rule this mood had to be worked up by others: it did not arrive spontaneously. Generally he saw the best in people; and, like Lord Kelvin, was able to disentangle ideas of value from the crude efforts at presentation of a beginner or of an ordinary muddle-headed man.

Gradually, as he grew older, the sense of public duty grew upon him, and he was prepared to spend his time in public service to an extraordinary, and as some thought a wasteful, extent. In 1888 he was appointed a member of the Board of Irish National Education, and devoted a large amount of time to work not free from controversy; and shortly before his death he was appointed, with five others, to the Intermediate Education Board. Had he lived (he has written to his uncle, Dr. Johnstone Stoney) he would have sought to devote himself to the organisation of National Education rather than to the uninterrupted pursuit of his science,—saying with complete sincerity that whether the human race got to know about the ether now or fifty years hence was a small matter, but whether the present state of appalling scientific ignorance was to continue for another generation was a vital matter affecting the future of his own country in a positive and definite way.

The portentous backwardness of this country (not Ireland alone) in education does indeed call for sacrifice on the part of those who clearly realise it; and into this work Fitz Gerald would undoubtedly have thrown himself. Until a general level of scientific knowledge has been attained by a nation, it cannot expect its great men to forge on ahead and continue their advanced studies with satisfaction to themselves. Already they have been feeling too isolated and aloof from humanity; and a feeling of the futility of it all, based upon the entire uncomprehension of the multitude—an uncomprehension shared under our present system of education by the great bulk of so-called educated men,—is apt to make itself unpleasantly prominent every now and then, and to lead gradually to the belief, at which Fitz Gerald

arrived, that greater service could be done by working towards the raising of the general level than by a pioneering quest, solitary or with only a few like-minded spirits, into lands too far removed from human traffic to be capable of utilization and absorption for generations to come; perhaps, therefore, to be forgotten and ignored altogether, until re-discovered independently hereafter, at a time when the general level of intelligence in scientific directions shall be higher than it is now, and can enable it to be appreciated and retained.

. . . . . .

Fitz Gerald was elected a Fellow of the Royal Society in 1883, and in 1899 was awarded a Royal Medal. He married in 1885 Harriette M., second daughter of the late Rev. J. H. Jellett, D.D., Provost of Trinity College, Dublin, to whom the next previous award to an Irish man of Science of a Royal Medal had been made. One who had unrivalled opportunities of appreciating these two men remarks on "the great likeness in the two characters: the great simplicity, the directness of purpose, the utter absence of preaching but the living of the life that is best; their great tenderness and love of children."

The "idealistic" turn of his mind in dealing with ultimate questions came out constantly in his conversation on such topics, and may be illustrated by a quotation from the end of his Helmholtz Lecture. After noting that all forms of external stimulus, into whatever terms we translate them—sound, colour, and the rest, nay, even space, time, and substance too, perhaps—resolve themselves into motion, he goes on to ask: "And what is the inner aspect of motion? In the only place where we can hope to answer this question, in our brains, thought [turns out to be] the internal aspect of motion. Is it not reasonable to hold, with the great and good Bishop Berkeley, that thought underlies all motion?" . . . . . "For the highest life we require the highest ideal of the Universe to work in. Can any higher exist than that, as language is a motion expressing to others our thoughts, so Nature is a language expressing thoughts, if we learn but to read them."

He insisted on the ether being not a simple fluid, with the atoms as vortex rings, but a medium itself full of motion—a vortex "sponge" or assemblage of vortex filaments; and by help of such a medium he hoped ultimately to be able to explain not only light and electricity but the structure and properties of matter, all its physical and chemical

agencies, and the material universe generally. But always he was well aware that such would be no ultimate explanation, that what we are really and primarily aware of is mind and mental processes, that thought and feeling are primary facts of consciousness, while all else is an inference and is probably essentially unlike what it appears to our senses: so that all this cosmic whirl and material activity, and probably life itself, would resolve itself, when properly comprehended, into the activity of an all-pervading and beneficent Mind.

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THE eminent scientific man whose work we here propose to review was born in 1851, second son of William Fitz Gerald, a distinguished theologian and metaphysician, Bishop of Cork, and afterwards of Killaloe. His early education was conducted at home, in company with his brothers and sisters, under the tuition of a lady who was for many years a valued member of the household, a sister of the great mathematician and logician George Boole, who was professor in the Queen's College at Cork. He was good at all subjects requiring close observation, from his earliest years. On passing to Trinity College, Dublin, at sixteen years of age, he soon made his way to the top in the classes of his year, obtaining his degree in 1871 at the head of the lists both in Mathematical and in Experimental Science. He became expert at gymnastic exercises and was for many years a fine racquet player; he also took a considerable part in the literary and social clubs of the College. His athletic skill was turned to account a few years ago in experimenting on the mechanics of flight.

The years from 1871 to 1877 were spent in reading with a view to the examination for Fellowship: vacancies were at that time very rare, but he was successful on his second time of trying. The course of study contained portions of the classical writings of Lagrange and Laplace, Hamilton and Mac Cullagh; and the close scrutiny of the modes of thought of masters such as these doubtless formed an admirable preparation for his subsequent scientific career. During this period he also made a formal study of metaphysics; he was much attracted by the idealism of his countryman, Bishop Berkeley, which kept cropping up in his discourses as the connecting link between material science and higher things, up to the end of his life.

It formed one of the manifestations of a poetic and deeply religious temperament.

For some years after obtaining Fellowship, he served with great success as a College tutor; his side was very popular, so that it was always full a long time in advance; and his services were in great demand as a director of the social and athletic organizations of the College. He gave up tutorial work on being elected in 1881 to the Chair of Natural and Experimental Philosophy, which he retained until his death last February. The care of a laboratory not adequately endowed, and a large amount of teaching of elementary classes, took up most of his time; what remained was much broken up in acting as Examiner in Physics in other Universities, and as referee of scientific memoirs for learned Societies, in copious correspondence with scientific friends who were sure of obtaining acute and stimulating opinions on all knotty points submitted to him, and in the discharge of public duties, which of late years had been increasing largely in extent and importance. Yet he found time to become one of the most learned men of the day in all branches of physical science, both theoretical and applied, and to leave his mark on the scientific progress of his age.

The physical problem attacked in his first published paper (*Roy. Soc. Proc.* 1876) was the then recently announced Kerr effect, the displacement of the plane of polarization of light on reflection from a magnet. By an argument based on general considerations, he connects this phenomenon with the double refraction indicated by the different velocities of propagation of right- and left-handed circular vibrations in magnetized media, as Stokes and Kelvin had already independently suggested. The details of the argument are corrected in the papers next following; but by a rough numerical estimate he shows, as Kundt experimentally verified some years after, that the Faraday rotatory power in traversing a film of magnetized iron must be very great, and conversely that the effect similar to Kerr's which must exist on reflection from a naturally rotatory substance such as quartz would be actually inappreciable. By neat and simple appliances he repeats Kerr's experiment; he moreover finds that the polarization really becomes elliptic, so that there is a phase-difference involved in addition to the rotation: in further confirmation that the effect is due to the magnetized substance, and not to the field, he finds that it is entirely cut off when a film of gold-leaf is spread over the reflecting

pole of the magnet. At first he thought that this elliptic polarization was directly involved in the double refraction; but in the next memoir he recognized that the difference of phase must arise, like the elliptic polarization of ordinary metallic reflection, "from the efficient reflecting surface being of some depth."

The subject of the connexion of light with magnetism was thus opened out, as a necessary preliminary to a more definite treatment of this problem of magnetic reflection; and, being led thereby into Maxwell's electric theory, he follows it up two years later in a memoir "On the electromagnetic theory of the reflection and refraction of Light" (*Phil. Trans.* 1880, pp. 691–711), presented to the Royal Society at the end of 1878, which constitutes his most complete formal treatise. Maxwell, in his memoirs on electrodynamics, and particularly in the definitive exposition published in *Phil. Trans.* 1865, had shown that the hypothesis that light consists of electric waves gives a straightforward account of the mode of its propagation in crystals, and one which is free from the difficulties attaching to the existing mechanical theories; yet he had not made any explicit application to the dynamical laws of reflection. It was left for Helmholtz to make the remark in 1870, in a footnote to his first formal memoir on the theory of electrodynamics (*Gesamm. Abhandl.* i, p. 558), that in this respect also Maxwell's theory leads directly to the observed results by supplying the appropriate boundary conditions at the transition between the optical media, whereas it had proved to be impossible legitimately to adjust any definite mechanical theory so as to admit those conditions. This statement was confirmed in a detailed formal investigation published by H. A. Lorentz in 1877. The subject of reflection is not noticed by Maxwell either in the *Phil. Trans.* memoir of 1865 or in his Treatise, which appeared in 1873, two years after the date of Helmholtz's memoir It seems possible, from various incidental remarks, that he was then content with the evidence afforded by the correspondence of the velocity of electric propagation in a vacuum with that of light; and that numerical discrepancies, such as that between the index of refraction in transparent bodies and the value of the square root of the specific inductive capacity, and the fact that metallic sheets are less opaque than their conductivities for steady currents appeared to demand, suggested that a closer view of the forced vibrations of the individual material molecules and their interaction with the æther would have to be acquired before the dynamical

problem of optical refraction became ripe for definite treatment. He was in fact in full possession as early as 1869, as Lord Rayleigh has recently shown, of the manner in which the phenomena of normal and anomalous dispersion are dominated by the influence of sympathetic vibrations in the molecules of the material medium. The analysis of this dynamical problem of optical reflection and refraction that was now completely developed by Fitz Gerald, in ignorance of Helmholtz's remark of 1870, and of the investigation by Lorentz which had just appeared, is of high import from the point of view of general electrodynamic theory. He first announces the proposition (most pointedly in the abstract of his memoir, *Roy. Soc. Proc.* 1879) that by introducing a vector (ξ, η, ζ) representing the magnitude of the total disturbance of the element of æther (of whatever nature it may really be) of which the magnetic force constitutes the time-rate of growth or velocity, the electrodynamic scheme of Maxwell, for dielectric media of æolotropic quality however general, can be immediately reduced to the standard form required by the principles of analytical dynamics; namely, the effective kinetic energy T and the potential energy W of the medium being given by the expressions*

$$T = \frac{\mu}{8\pi} \int (\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2)\, d\tau$$

$$W = \tfrac{1}{2} \int U d\tau,$$

where $\delta\tau$ is an element of volume and U is a quadratic function of (f, g, h) characteristic of the material medium, (f, g, h) representing the curl of (ξ, η, ζ) or

$$\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}, \quad \frac{d\xi}{dz} - \frac{d\zeta}{dx}, \quad \frac{d\eta}{dx} - \frac{d\xi}{dy}\right),$$

then the entire system of equations of electromotive propagation is obtained in the Lagrangian manner by annulling the variation of the Action, that is from the *single relation*

$$\delta \int (T - W)\, dt = 0.$$

There are, however, other analytical expressions for T and W which would lead to the same equations of bodily propagation, as witness the various elastic mechanical theories of light which give the correct

* In case the medium is not homogeneous μ would be inside the integral.

laws of transmission; in fact by integration by parts we may express $T - W$ in the form of a new volume integral, together with a surface integral extended over the boundary of the region considered, and of these only the former part is concerned in the formation of equations of propagation, though the latter is essential to the relation connecting stress and strain.

Fitz Gerald points out, following his countryman Mac Cullagh, and also Green in a different connexion, that not merely the equations of bodily propagation, but the equations of transition expressing continuity of stress and displacement between the media at the reflecting interface, are definitely and consistently involved in this way of formulating the problem by a single equation, the surface integrals in the reduced variation of the action determining the latter relations. He then remarks that the electric theory, as thus expressed in terms of this magnetic displacement (ξ, η, ζ), is formally identical with Mac Cullagh's dynamical theory of crystalline reflection and refraction published thirty years before. The problem which Mac Cullagh then proposed to himself was to work backward from an analysis of the actual circumstances of crystalline reflection, as determined mainly through the experiments of Brewster and Seebeck, to a type of Lagrangian functions $T - W$ for the two media of propagation which would correspond in their consequences with these very various test phenomena. It has always been admitted that he was entirely successful in this analytical quest: but he definitely proclaimed his inability to conceive of any kind of material elastic medium, as ordinarily understood, whose properties would be represented by the relations finally obtained. The reduction of the optical phenomena under the analytical type to which all dynamical processes must conform was successfully achieved; but it proved to be impossible to trace a close analogy with any of the other types of dynamical propagation that had at that time been explored. The fact that, in any material medium, in which dynamical phenomena are determined entirely by interaction between internal stress and forces of the usual non-polar kind acting from a distance, there would, on Mac Cullagh's scheme, be an unbalanced torque on each element of volume, shows that a mechanical theory of this type can subsist only if there are polar forces (*e.g. quasi*-magnetic) capable of compensating the torque, or if there is a kinetic reacting torque arising from a distribution of gyrostatic rotations forming a part of the constitution of the medium.

In later years Lord Kelvin showed how to construct a cellular material system with gyrostats mounted in its elements, which would form an actual model of this rotationally elastic medium of Mac Cullagh. The hasty assumption that the analysis of the latter must necessarily be identical with the tentative mechanical theory of F. E. Neumann, because it gave the same results, had led to the real significance of Mac Cullagh's investigation being overlooked at the time; doubtless, too, it was felt that the possibilities of purely mechanical media of ordinary type had not yet been exhausted. It was accordingly an advance of prime importance when Fitz Gerald pointed out, in 1878, that the strain, of unknown nature but known relations, which Mac Cullagh's theory required, was precisely the one, equally unknown to our perceptions of sense, on which Maxwell's electro-optic theory is based; his memoir, in fact, terminates with the weighty remark that Maxwell's theory, if it "induced us to emancipate our minds from the thraldom of a material ether, might possibly lead to most important results in the theoretic interpretation of nature."

Other considerations may be mentioned in illustration of the significance of Fitz Gerald's mode of considering the subject. In the statement of electrodynamic theory elaborated by Heaviside and by Hertz, which has been widely accepted as a powerful aid to concise formulation and development, the main feature has been the elimination of the potential functions of the vector electric current and the scalar electric distribution, that were introduced in the course of Maxwell's analysis: in Fitz Gerald's dynamical procedure, from the basis of the principle of Least Action, no such potentials occur. Indeed Maxwell had himself pointed out (*Phil. Trans.* 1868) that the equations of electric propagation were most compactly expressed (when their theoretical basis was not in question) by the pair of circuital relations between the fundamental vectors which result from the elimination of these potentials. In Fitz Gerald's analysis the relation in which the interfacial conditions stand to these bodily equations is moreover directly involved.* The problem as treated by him was, however, limited expressly to the case of transparent media, in which there are no wandering electric charges. The natural way

* In later years Hertz initiated attempts, not wholly successful, (previously however essayed by L. Lorenz) to deduce the interfacial equations from the bodily equations of the medium alone, by means of a hypothesis of physical continuity across a thin layer of very rapid continuous transition between the media.

to extend his method to the more general questions in which charges are in finite translatory motion, either from conduction or by convection of the material system, is to retain his analytical specification of $T - W$ for the free æther, but to introduce into the process of variation the conditions imposed by the presence and persistence of these electrons; in the reduction of this more general problem to differential analysis suitable for material media treated as continuous, the employment of Maxwell's potentials can hardly be evaded.*

The other general remark is that, in the question as to whether Maxwell's electrodynamic scheme is sufficiently wide to cover the facts, it would have been possible to reason from the optical problem of crystalline propagation already fully analysed by Green, Neumann, and MacCullagh, in the light of the very various and easily executed optical experiments that are available, to the more general electric problem in which crucial experiment was for a long time almost unattainably difficult. There was fair ground for the inference from MacCullagh's analysis that his scheme was the unique one of formally dynamical type that could be consistent with the laws of crystalline reflection: no demonstration of this was indeed given, but the variety of experimental laws to which the solution had to conform created a presumption almost equivalent to proof. That scheme might be made more general by assumption of effective compressibility of the medium, *i.e.* convergence of the magnetic vector in the electric interpretation, thereby introducing compressile waves; these would, however, travel by themselves in entire independence of the transverse waves. But a scheme such as Helmholtz's, which introduced to any sensible extent compressile waves interacting with the transverse ones, would in so far come into conflict with the optical evidence. This question, whether Helmholtz's proposed generalization of Maxwell's theory had any actual basis, remained a main problem of theoretical electrodynamics until the discovery of electric radiation by Hertz: it has now been tacitly dropped, not so much on account of any specific experiment in disproof, as through the conviction, created by the ever-widening range of correspondence between electric and optical radiations, that these are the same thing at bottom. Towards this conviction the illustrative experiments of FitzGerald and his pupils had their share in contributing. It does not appear that he ever paid

* Cf. Larmor, 'Æther and Matter' 1900, or *Camb. Phil. Trans.*, Stokes Memorial Volume, 1899.

much attention to the Helmholtz theory: it reposed ultimately on ideas involving action at a distance, and from the optical point of view which we have here attempted to sketch there was no room for its wider generality.

The extension of this theory of optical reflection to the magneto-optic problem was effected by Fitz Gerald by introducing, after Maxwell, terms into the function $T - W$ so as to represent interaction between the imposed magnetic field and the optical vibrations in the medium. He finds, however, that the interfacial conditions in the dynamical problem could not now be satisfied by means of transverse waves alone; and, reserving the theoretical difficulty thence arising, he completes the analysis without introducing compressile waves by neglecting the condition involving the normal disturbances, as Fresnel had done under similar circumstances. This gives him a magneto-optic effect resembling in details, as regards intensity, the description published by Kerr in the same year, but without changes of phase.

Finally it is pointed out that this is only a first approximation to an exact theory, on account of the metallic character of the reflector being ignored, and of the difficulty above mentioned—the question of phase in particular awaiting a more comprehensive theory, in which the relation of matter to æther would be more exactly expressed. The difficulty has since been removed by considering the effect of the imposed magnetic field to be a structural modification in the material medium, rather than a fundamental change in the dynamical relation between the electric vectors; and Fitz Gerald's analytical work still remains applicable.* The extension to metallic media is now formally made, as Ohm's laws indicates, by taking the refractive index to be a complex quantity: with this generalization the analysis has been extended by various writers, including Lorentz, Goldhammer, and Drude, but most completely by Leathem† and by Wind,‡ and shown to embrace satisfactorily all the mass of detail that has been brought out in recent years in experimental magneto-optic investigations.

* In one of the papers on his mechanical model of the electrodynamic field, of date 1885 (*infra*, p. 162), Fitz Gerald had already expressly distinguished between these two methods of accounting for magneto-optic rotation; the distinction between them is important mainly in the above connexion.

† *Phil. Trans.* 1897.

‡ *Archives néerlandaises*, 1897; or *Physical Review*, Vol. vi.

While these developments were proceeding, Fitz Gerald was also taking a close interest, stimulated in part by his uncle, Dr. Johnstone Stoney, in the theory of the Crookes radiometer, then coming into shape; this he studied in its connexion with the cognate subject of the constitution of the gaseous layer which supports a liquid in the spheroidal state without contact with the hot slab, and the state of stress that is as a consequence involved in it. In an elegant subsidiary investigation (March 1888) of the state of internal pressure in a gas, arising from supposed presence of some co-ordination in the directions of motion of the molecules, he finds that, when the directions are simply related to a single axis,* an internal stress of the Maxwell electrostatic type results; in the general case the stress is not specially conditioned. In Fitz Gerald's point of view the normal pressure on the warm side of a vane of a radiometer may differ from that on a perpendicular unheated side, because the stress in the gaseous medium is not isotropic; how far this mode of representation is consistent with the more complete molecular analysis of internal stress in gases due to temperature-gradient, effected by O. Reynolds and by Maxwell, has been a subject of controversy.†

* This is on the assumption that the relation to the axis is of a non-polar character. If the axis were that of a temperature gradient, there would also be polar quality in the distribution of molecular motions, involving greater velocities in one direction than in the opposite one; this would require a rising gradient of density down the axis of temperature in order to dynamically compensate it.

In later years Fitz Gerald was sometimes led, by the above analogy, to imagine the hypothetical Faraday-Maxwell electrostatic stress as transmitted by an æther consisting of independently moving corpuscles of the Le Sage type, whose directions of motion somehow became related in a non-polar manner to the directions of the lines of electric force.

It may readily be shown, after the manner of Maxwell's 'Electricity,' § 106, that if a medium is subject to a tension of intensity R^2 along a system of curved lines, accompanied by an equal pressure in all directions at right angles to them, this stress cannot be in internal equilibrium unless R represents the velocity of an incompressible fluid for which these curves are the lines of flow, and at the same time the flow is either irrotational or else the lines of vorticity coincide with the lines of flow. The former of these alternatives coincides with the electrostatic application.

† A detailed criticism of Prof. Reynolds' Memoir of 1879, contributed by Fitz Gerald to the *Phil. Mag.*, has not been reprinted in this collection. The following passage from Prof. Reynolds' rejoinder, *Phil. Mag.* May, 1881, appears to bring out the disputed points:—

"It seems that Mr. Fitz Gerald is of opinion that the parallel flow of heat does

In 1881 a new departure was initiated in theoretical electrodynamics by J. J. Thomson, in an investigation with the aim of determining directly the effects produced by moving charged bodies, by means of Maxwell's equations of the electric field combined with the appropriate conditions at the surfaces of the moving conductors. The question was whether the hypothesis that the moving charge acts after the manner of a current-element is an independent assumption, or to what extent it is already implied in Maxwell's scheme of the electric field for bodies at rest. Neglecting the self-induction of the æthereal displacement-currents surrounding a moving charged sphere, that is, assuming instantaneous transmission of effects, it was shown that the continual adjustment of the electric force in the field, so as to be normal to the conducting surface as it moved through space, entailed a magnetic field of the type demanded by that hypothesis. Thus, if the charged body carries its electric field along with it, the same as if it were at rest, that convection creates the magnetic field aforesaid. At speeds comparable with the velocity of radiation, the result of taking account of the finite velocity of the electrodynamic transmission in the analysis is that the electric field thus convected, when the steady state of disturbance has become fully established around the moving conductor, is somewhat flattened equatorially, and the magnetic field is correspondingly changed: but ordinary electrodynamics based on the convection of electrons can (and does) safely leave such extreme cases aside. An important result in this paper was the remark that this

cause stresses in the gas, and that he has been trying to find that I have not disproved the possibility of such stresses. If he confine his attention to stresses of the same order of magnitude as those now shown to exist in the case of converging or diverging flow, he will find that both Professor Maxwell and I have proved the impossibility of their existence; but if he go, as he appears unwittingly to have done, to a higher order of small quantities, then I have nothing to say, except that he has no inconsiderable task before him. . . . I have never asserted that the variation of pressure in the direction of the flow of heat, which I have consistently maintained to be necessary to the production of the phenomena of impulsion, may not be attended by a difference of pressure in different directions; and, of course, I have known that such must be the case since the time that I have seen and proved by experiment that this direct variation of the pressure depends on the convergence of the lines of flow. . . . But what I have consistently maintained is, that a difference of pressure in different directions (*i.e.* parallel and normal to the hot and cold surface) will not explain the experimental results, and this was the theory advanced in opposition to mine, and which Mr. Fitz Gerald still seems inclined to defend."

field of magnetic (kinetic) energy carried along by an electron involves an addition to its effective mass; the question has in fact more recently been propounded whether all inertia of matter may not be of cognate type. The paper took its rise from the magnetic deflection of the cathode stream in a vacuum tube, as already at that time precisely described by Crookes; and, when some of the analytical computations had been corrected by Heaviside, this memoir of Thomson's constituted the beginnings of what has now become the basis of electrodynamic theory. It is interesting also to observe in it the suggestion that the nearly abrupt changes in the magnetic field arising from the sudden stoppage of the cathode particles may be responsible for the phosphorescence excited by them where they strike the glass: how much more they are actually responsible for was revealed years after through an accidental observation of Röntgen's, after the previously unsuspected magnitude of their velocity had been ascertained by J. J. Thomson. In a critical paper (No. 20 *infra*) Fitz Gerald had something to add: he pointed out that as the spherical conductor of finite size, to which the analysis related, moved along, the field of æthereal elastic displacement is suppressed in the space that the sphere comes to occupy and is instantaneously established in the space it leaves behind: these changes constitute impulsive displacement-currents: when their vector potential is added to that of the displacement-currents of the field, the result does not give the correct magnetic force—it gives in fact none at all—the current of convection of each superficial element of the charge on the sphere has to be included as well. Thomson, without explicitly including the latter, had done what comes to the same thing by addition of a term required to make the vector potential circuital, thereby implying that the æthereal current must be supplemented so as to make it everywhere circuital also. Thus on closer examination, each portion of the electric charge is found to act independently; and so far from our being able to exclude the electric charges from view by merging them in interfacial conditions, it turns out that their convection is the sole cause of the phenomena of the electric field. When considered from the point of view of the æther, the hypothesis that the total current is circuital, which lies at the very root of Maxwell's theory, involves and is equivalent to the magnetic influence of moving charges, which was verified experimentally before this time by Rowland, though doubts still occasionally arise on the part of unsuccessful experimenters. Following

out the same train of thought, Fitz Gerald soon after introduced order into an obscure passage in Maxwell's exposition in his 'Treatise,' by inserting the term representing the current due to the convection of charged bodies, which had in that connexion been overlooked.

In 1882 he contributed three papers to the Royal Dublin Society on the conditions necessary to excite electric radiation; at first he was led into inconsistencies by some confusion between the true exciting cause of the radiation, existing in the radiating matter alone, and the displacement-currents in the surrounding æther to which Maxwell formally ascribes like properties. But he gradually gained firm footing, and began to think of the vector potential as something *propagated in time* from the true electric sources, yet remaining essentially the same function which Maxwell treated as an *instantaneous* potential of the true currents and the fictitious æthereal currents taken together. He concludes that the production of radiation by electrical means is possible, that "the interactions between the molecules of matter and the æther are of the same character as the electromagnetic actions with which we are acquainted"; and at this early date he goes on to express the opinion that the discharge of a condenser, arranged so as to have very little inductance in the discharging circuit, may be made to send out actual electric radiation of manageable wave-length.

Following up this train of ideas he published towards the end of 1883 an investigation of the intensity of radiation that would be electrically obtainable. His radiator was an alternating electric current flowing in a small circuit, or, as he afterwards described it, a vibrating magnetic doublet, in contrast with the later electric doublets of Hertz. In this brief but for the time very remarkable paper, he starts with an expression for the vector potential A as *propagated* from the varying true current $i_0 f(t)$ in the linear circuit, namely the vector formula

$$A = \mu i_0 \int \frac{f(t - r/c)}{r}\, ds;$$

then calculating the kinetic energy in the surrounding field by the formula

$$T = \tfrac{1}{2} \int (Fu + Gv + Hw)\, d\tau,$$

where (u, v, w) is now total current, and $\delta\tau$ an element of volume, he finds that in the spherical shell between radii R and $R + \delta R$, this involves an amount of energy which is proportional to δR simply

together with another amount which also varies as R^{-2}. The latter part he identifies as belonging to the "forced displacement-currents" around the vibrator, that is to the oscillations of the vibrating æthereal system at the source, which send out the radiated displacement-currents of which the former term represents the energy. The energy thus radiated per second comes out as

$$\tfrac{8}{3}\pi^4 (i_0 A)^2 \frac{\mu}{c^3 T^4}$$

where A is the area of circuit, T the period of the alternations of the current when they are simple harmonic, and c the velocity of light. He points out that all of this formula except the numerical factor could have been obtained from the theory of dimensions alone, assuming of course the existence of clear ideas of the physical process involved. This radiated energy is very small in amount except for high frequencies; with 10^7 alternations of the current per second it amounts $\frac{1}{10}(i_0 A)^2$ ergs per second. He suggests the possibility of estimating the magnetic moment of a gaseous molecule considered as a vibrating magnetic doublet, in this manner, from a knowledge of the radiating power of the gas. He had here not quite got to the conception of identifying the source of radiation with the motions of the electrons in the molecule. Hertz afterwards approached closer when he took for his theoretical vibrator an alternating electric doublet instead of a magnetic one.*

As everybody knows, the apotheosis of these studies of radiation, so clearly presented to the mental eye, came three years later in Hertz's detection of the phenomena† which have now, in wireless telegraphs, become developed into a commonplace of civilization. Beyond all question the genius of Hertz deserved this success, and he stands alone. But the tracing of the history of scientific thought on the eve of a great advance is always fascinating and instructive. At the very time Fitz Gerald was formulating these profound yet direct views, or soon after, Oliver Lodge, his close friend and in a sense his pupil, was actually engaged in feeling out the nodes of the electric waves that ran along his discharge-circuits like sound-waves

* Cf. *infra*, pp. 272–273, and in other places. Reference may also be made to the simple zoetropic reproduction (p. 275 *infra*), previously suggested by Hertz, of the features of the propagation of the lines of electric force from the radiator.

† First expounded in this country by Fitz Gerald (No. 49, *infra*).

along pipes; and there can be little doubt that the goal of the experimental demonstration of Maxwell's theory would have been reached along this path, though not perhaps with the same directness as Hertz attained. Indeed it appeared that phenomena of the same kind as Hertz's had been noticed long before by Joseph Henry, and had suggested thoughts of propagation of electric influence; now when we are instructed, the difficulty is so to speak to avoid them.

Referring again to this paper on the intensity of radiation, we see clearly emerging from its analysis the conception of an electric source, and of the electric field as formulated in terms of vector and scalar potentials directly propagated from the sources. This is the idea which H. A. Lorentz developed into a general theory of electrodynamics in 1892, ten years afterwards. The expression of his results has attained to ultimate simplicity in the compact statement by Levi-Civita in 1897, that vector potential, varying as usual inversely as the distance, continually travels out in time, with the velocity of radiation, from each element of true current, and change of static electric potential similarly travels out from each displacement of electric charge, and that the electric force (on electricity at rest) and the magnetic force in the æther are expressed in terms of these potentials in Maxwell's manner.* But this kind of vector potential is a difficult one for purposes of analysis of media treated as continuous, its value at a point at a distance r from a source depending on the strength of the source not at the instant considered but at a previous

* The very same scheme had been proposed as early as 1867 by L. Lorenz, as a modification of the Weber-Neumann equations which would equally well represent the facts of electrodynamics for bodies at rest so far as at that time known, while it would also include an electric theory of light consistent with existing optical knowledge. Cf. Lorentz, *Pogg. Ann.* 1867, 'Œuvres Scientifiques' i, p. 179, referred to by Maxwell, 'Treatise,' ii, § 805. This brilliant and powerful attempt at generalization, though precisely parallel analytically with the form of Maxwell's theory referred to in the text, cannot however be considered as an independent physical advance to that theory from a different point of view, the physical foundation on which it was based by its author being in fact self-contradictory. All true current was taken to be current of conduction, dielectric polarization not being contemplated; thus even in free space it was necessary to have conductivity, otherwise true current could never become established there. Yet if a distribution of current could be supposed thus established, it would travel according to the same laws as the light-vibration, as a result of the initial postulate that all the effects are propagated in time with a common velocity,—provided however that the conductivity is small enough to be neglected in this latter connexion. But on the other hand, if the conductivity is indefinitely small it could never give rise to any electric flux to be so propagated; thus the hypothesis of free propagation in time is

time—previous by r/c when the source is at rest. In practice, Maxwell's original potentials are much more convenient: with him there is no finite propagation of potentials, their values depending on the strengths of the sources at the very instant considered, but to attain to this simplicity the change of so-called æthereal displacement must be treated as current in Maxwell's manner, as well as the convection of the charges. It is only for the total current thus complexly constituted that the simple laws of the early electrodynamics of Ampère and Faraday are strictly applicable, when the incomplete circuits of electric discharges are under consideration. A theory which developed itself on the basis of the universality of these laws was thus bound to follow Maxwell's mode of exposition. In 1893 the formal development of electrodynamic theory from this point of view, inspired at the start by Fitz Gerald's electric interpretation of Mac Cullagh's optical analysis, and by mechanical models based on Lord Kelvin's construction for a rotationally elastic æther, and resting directly on the single broad dynamical basis of the principle of Least Action—the molecular character of all electrification and the recognition of the physical nature of the effects produced by its motion being an indispensable feature—was initiated in Great Britain; it was practically complete early in 1897,* running parallel in the

inconsistent with conduction. The theory of Maxwell requires the recognition of polarization currents in dielectrics as well as currents of conduction, thus replacing Lorenz's coefficient k by $k + k'd/dt$; it also requires in its more detailed development, as indicated in the text, that what is propagated across empty space is not a disturbance of electricity but a wave of elastic displacement of the æther. The mere introduction of a dielectric constant (implied in k') for matter and also for æther, in addition to Lorenz's conduction constant (k), would, however, at once bring his theory into conformity with Maxwell's, at the stage in which the latter was left by its author.

* Larmor, 'A Dynamical Theory of the Electric and Luminiferous Medium,' *Phil. Trans.*, 1894 A, pp. 719–822; 1895 A, pp. 695–743; 1897 A, pp. 205–300; also 'Æther and Matter,' Camb. Univ. Press, 1900. The general conception that the atom is not a mere mass-point, but is the centre of a correlated field of power in the surrounding æther, was fully realized by Faraday; while its germ doubtless goes back to Descartes. Cf. Faraday's 'Experimental Researches,' vol. ii, pp. 284–93, vol. iii, p. 20, in connexion with his magneto-optic discoveries, also a vivid general argument in vol. iii, pp. 570–4, and the various papers on physical lines of force contained in that volume. What remained to be accomplished was detailed mathematical development along the lines of Maxwell's interpretation of Faraday's views of electric transmission, on the basis that the field of power of the atom is, wholly or largely, the intrinsic field of its electrons.

main results, though not in method or mode of development, with Lorentz's final presentation in his tract of date 1895. This mode of investigation immediately obtained the strong and active support of FitzGerald. The discovery by Zeeman and Lorentz in 1897 of the subdivision of the lines in a spectrum by a magnetic field afterwards provided the stimulus required to attract general attention to a theory whose logical processes were difficult and unfamiliar, in whatever manner it was developed: so that this point of view is now universally prevalent, and is perhaps occasionally pushed to extremes that would have discredited it when in the purely theoretical stage. The distinction here sketched between the analytical potentials of Maxwell and the potentials that are actually propagated from true electric sources had been, as may be gathered from the account here given, long ago fermenting in FitzGerald's mind, though it is only recently that it has emerged into the simplicity demanded for its general recognition: it appears from one of his last letters that it had been his intention to work the subject up, if time had allowed, into a paper for the British Association in September, 1900.

Fitz Gerald's skill in intuitional construction was illustrated by a striking model of the functions of the æther, invented in 1885 in connexion with the dynamical theory of his memoir on optical reflexion. The problem was to form a simple representation of a medium possessing the type of elasticity, rotational but not at all distortional, described above. He effected this, as regards two dimensions of space, by mounting a number of wheels freely on a board, and connecting adjacent ones in pairs in various ways by elastic bands stretched over them, so that, as the bands maintained a hold, the wheels became bound together into a single elastic system. Fixity of the centres of the wheels implied incompressibility of the medium. A rotation imparted to one of the wheels spread itself to the surrounding ones and at the same time set up elastic stress in the bands: this stress represented a field of electric or æthereal polarity—the poles being the two sides of a band, one side extended, the other shortened. A rotational disturbance imposed at any place in such a medium was propagated by the elasticity of the bands, with finite speed on account of the finite moments of inertia of the wheels: the kinetic energy of the revolving wheels represented the magnetic energy of the field, which was thus in direct relation to the changing electric strain. In the state of equilibrium, free from internal polarization, the elastic

bands must be supposed to be free from such differences of tension between their two sides. A region in which the wheels were loose represented a conductor. An intrinsic polarity may be introduced into this medium at any place by rotating one or more of the wheels there situated, making them slip underneath the bands that pass around them: when they are released there will be a permanent field of strain, *i.e.* an electric field. The lines of electric polarity will be circles surrounding the place, which therefore is the analogue of a changing free magnetic pole; an electron is not readily represented, nor therefore are electric currents which involve the transfer of charges through the medium. But we can have currents within a limited range; if the region contains perfect conductors, slip is continually taking place at their surfaces when disturbances are going on in the medium; this represents a current sheet around the surface of each conductor, which compensates the external disturbance and so avoids any penetration to the inside of the conductors. The model in fact illustrates the phenomena of dielectric propagation, which is necessarily of alternating character; but it does not include the free mobility of electric charges which is involved in an ordinary current of conduction. By employing wheels with various orientations, and replacing the elastic bands by hydraulic connexions, the model is capable of extension to the three dimensions of space.

The converse (in a sense) of this plan, which took the kinetic, *i.e.* magnetic, energy of the medium to be the rotatory energy of the wheels, gives the rotationally elastic medium of Rankine and Kelvin, with kinetic energy translatory, which can be practically illustrated only by complicated gyrostatic systems. In each case the elastic energy is rotational. But here electrons can be definitely created in the medium by supernatural operations involving a kind of knotting process,* while free magnetic poles cannot exist; and a true electric current being a flux of electrons, the representation is complete. Not, however, entirely complete; for the nature of the mobility through the medium of the core of the electron is unspecified, though when that is postulated the model shows how its surrounding electric field necessarily travels along with the core. The nature of the core itself involves the problem of the intimate intrinsic structure of the material atom: if that could be explored there would be no secrets left in Nature: in electrodynamics and optics we may, therefore, be content

* Cf. Larmor, 'Æther and Matter,' Appendix E.

to know the way in which an electric field travels through the æther without ever becoming disconnected from the unknown nucleus or electron to which it belongs. The science of physics is concerned only with the interactions between the atoms, which operate through the agency of the æther in which they definitely subsist; we may attain to clear views on that subject even though we be warned off the more obscure, perhaps unfathomable, problems of the intimate nature of matter itself, and of the vital manifestations of which it can become the seat.

Examination of the very interesting papers devoted to this subject will help to elucidate the fundamental part which intuition, working by means of models and analogies, can play in the formation of physical theory, and the breadth and justness of the general views as to the operations of Nature that can thence be acquired. According to Helmholtz this method of procedure has been characteristic of the British School. Its advantage is that correlations between directed physical quantities, which for their mathematical expression may require intricate formal operations, are made to run parallel with actual processes with which we are conversant from experience, so that they can be considerably developed without much trouble; the transition from analogy to theory then proceeds through abstract logical analysis of these familiar processes, in the course of which ideas become permanently generalized, and in the end but little of the original analogy may remain. Notable examples are Cavendish's modes of thought which carried him some way into the nature of electrical phenomena more than a century ago, and the ideas of Faraday which formed the foundation of his own discoveries as well as the basis of the modern point of view in physical science. An intermediate link, between bare physical intuition and fully correlated theory, has been forged in the entirely new range of dynamical thought involved in Helmholtz's cardinal discovery (illustrated and doubtless suggested by the comparative permanence of eddies in actual fluids) that permanent vortex aggregates maintain their existence (subject to the condition necessary for stability) in a frictionless fluid medium, solely through the interaction of inertia with the implied perfect continuity of movement, that they in fact determine, and in a sense control, the entire motion of the medium in which they subsist.* And, taking its rise

* For some years past Fitz Gerald was considerably occupied with difficult but very suggestive speculations, in part published, explaining the elasticity of the æther by a tangled structure of vortex filaments in a medium otherwise fluid.

from special problems of gyrostats and vortices and other steady motions, we now possess the simple and masterful generalization of the Lagrangian dynamics above described, foreshadowed by Kelvin and Maxwell, developed independently by Kelvin and Routh, and subsequently expounded by Helmholtz, in which the conserved momenta of permanent internal motions are considered to be a part of the constitution of the system just as much as the distribution of the masses and the geometrical configuration. So that while Lagrange and Hamilton were able to form a synthesis of the possible modes of motion of a purely dynamical system, specified as regards mass and configuration, by means of the single formula of Least Action, we are now able, as the result of a still more concentrated analysis, to express the laws of the dynamical phenomena that can arise in a general system more closely specified by mass and configuration and intrinsic motions, and to lay down the limitations to which all such non-dissipative dynamical systems must conform. The question suggests itself whether, in face of so wide a synthesis, the *rôle* of analogies and models in the discovery of physical principles is not now ended: Helmholtz has in fact himself recorded that, in general physics, he found it easier to begin with wide formal relations into which the phenomena are to be fitted rather than to ascend through special analogies to more general views. Yet this descriptive method may, perhaps, still be held to be more suitable for codifying existing knowledge than for feeling after its underlying implications. Doubtless there are advances yet to come which will arrange in still clearer and simpler order the main features of the phenomena that can arise in purely dynamical systems, and the essential limitations to which such systems are subject.

In 1882, in a paper on the electromagnetic effects due to the motion of the Earth, Fitz Gerald turns his attention to the question how far Maxwell's scheme satisfies the very crucial test arising form the fact that we have here a velocity of as much as 20 miles per second available for our observations.* In his treatise Maxwell had noticed, somewhat vaguely, that the general form of his electrodynamic equations is not affected when the whole system is moving with uniform translatory velocity, if the axes of reference move along

* Later he remarked the bearing of this velocity on any hypothesis that would make inertia a function of temperature: any such addition to mass, that might arise from change of temperature, would demand a large impulse, such as could not fail to be noticed, in order to acquire for it the velocity of the Earth's motion.

with the system; and the optical consequences of this fact were doubtless present to his mind. If the æther were supposed to move along with the material system, this result would indeed be involved in the mere principles of relative motion. It is not true in any case for movements of rotation. In the paper under notice it is recognized that there would be an alteration in the force acting between electric charges, arising from their convection, for if they are convected alongside each other with the velocity of light their mutual attraction would disappear; for small velocities the alteration is however of the second order. But he now found from Maxwell's equations that there would be no effect produced on a moving magnet by any charges carried along with it, for there would be induced on its surface a charge which would exactly neutralize the magnetic influence of the moving external charges; and conversely there would be no force on the moving charges arising from currents or magnets which are carried along with them. In ignorance of this result, quite recently it has still been thought to be a difficulty in æther-theory, that no magnetic action of charges convected with the Earth's motion has been observed. In fact FitzGerald went about as far as was possible at that early date: the complete analysis of convection involves separate treatment of the moving electricity and the stagnant æther, such as was successfully developed by Lorentz and other writers ten years afterwards. Later on he was greatly interested in the discrepancy between Michelson's classical interference experiment, which suggested that the æther moved rigorously along with the Earth, and the body of electric doctrine which almost demanded that it should be stagnant. He was the first to suggest the natural reconciliation, for which much theoretical evidence has since accrued—which is in fact a necessary consequence, if forces of cohesion are wholly or mainly electric—that motion through the æther affects the dimensions of solid molecular aggregations, but to an extent so minute as to be detectable only by most refined optical measurement.

There is a paper of this date on comets' tails. It is pointed out that Maxwell's pressure of radiation, acting on cosmical dust, being proportional to the intensity of the radiation, is inversely as the square of the distance and so proportional for all distances to the gravitational attraction of the radiant body. For attraction thus to be changed into repulsion the mass must be very light, or its effective surface very great compared with its volume; the latter condition

may be attained by extreme smallness of the particles. The more minute analysis that is now possible connects the repulsion directly with reflection and absorption of the waves of radiant energy; in these processes a definite mechanical force is established whose reaction is on the surrounding æther and not on the radiant body. The validity of the principle of Least Action for the complex system æther *plus* matter implies that action and reaction are everywhere balanced for these two constituents taken together; but the Maxwellian pressure of radiation, now satisfactorily established on thermodynamic and other evidence, shows, as Poincaré has recently admitted, that this balance cannot always be maintained for either constituent separately.

In 1886, he published a study of the limits of the velocity of the working parts of engines, a subject which has come to the front recently in connexion with the steam turbines of Parsons and Laval.* In the same year, the work of Forel on the Lake of Geneva induced him to publish an account of a beautiful and physically very suggestive survey of the meteorological thermal phenomena in Lough Derg, which he had made while at home at Killaloe, in the summer of 1876, when he was still a candidate for Fellowship.

He was a powerful advocate (pp. 307-20) of the theory of Crookes and Stokes, that the cathode rays are streams of electrified particles, when the balance of opinion abroad was strongly against that view; and in 1893 he broached the view (p. 298) more recently developed by Arrhenius, that the outlying streamers of the Solar corona, and magnetic storms on the Earth, may arise from projection of jets of electrical ions from the Sun.

In recent years Fitz Gerald has taken a leading part in the critical discussions regarding the foundation of the laws of osmotic phenomena and electrolysis, and the ionization of solutions. Indeed this latter term, now universally adopted to distinguish the phenomena from ordinary dissociation, in which electric separation does not occur, was originally due to his suggestion; recently, in a contribution to a British Association discussion, abstracted afterwards in *Nature* (p. 523 *infra*), he proposed the term electronization for the still different phenomena of molecular decomposition that are involved in the formation of the cathode streams in vacuum tubes. Some years ago

* The same problems are treated by O. Reynolds in the *Engineer* in 1881, reprinted in Collected Papers, vol. ii.

he completed the work of Maxwell and Chrystal on the degree of validity of Ohm's law of conduction, by a minute test in electrolytes with currents conducted through a small hole in a diaphragm; no deviation was detected in the resistance required to balance his bridge, whatever was the current employed, so long as the temperature remained the same; and the law possibly holds rigorously up to the point of rupture of the conducting medium.

The slight displacement of the lines in a spectrum towards the red, with increasing pressure of the surrounding gas, which was discovered and investigated by the Baltimore spectroscopists, he ascribed to increase of density of the surrounding matter rather than to pressure. Each molecule in the neighbourhood contributes to reduce the effective statical elasticity of the vibrating æthereal system, in a way which when averaged up on a large scale shows itself as increase of the electric permittivity of the medium as a whole; thus the presence of each molecule must increase all the periods of free vibration of neighbouring ones: in fact Wilsing and others have observed that when the spark is taken in dense liquid media, the displacement of the spectral lines becomes large and indefinite. He has also paid much attention to the Zeeman change produced by a magnetic field, and was the first to develope in a definite quantitative manner (p. 464 *infra*) its relation to the Faraday magneto-optic effect.

An account of an experiment bearing on the refined and delicate subject of the relation of æther to matter, which FitzGerald instituted with Dr. Trouton during the last months of his life, and which has been carried out by the latter, is reprinted (No. 107 *infra*) from Dr. Trouton's memoir. There is weighty theoretical evidence* that, at any rate if the atoms of matter are constituted electrically, uniform translation of a material system through the æther does not alter its mechanical, electrical, and optical phenomena, up to and including the order of the square of the ratio of its velocity of translation to the velocity of radiation; because, in fact, the molecules of the system adjust themselves to the new conditions by the FitzGerald-Lorentz change of dimensions above mentioned. Any motion not uniform, but in which the change of velocity is slight in the time required for electrodynamic disturbance to become adjusted by propagation across the system, comes practically within this rule. FitzGerald seems to have thought (No. 101 *infra*) that a motion abrupt enough to be out-

* Cf. Larmor, 'Æther and Matter,' chapters ix-xi.

side the rule, and so possibly to reveal mechanical interaction between æther and matter, might be available in the spark-discharge of a condenser; when Crémieu's difficulties in verifying the Rowland convective effect were first brought forward, he had an idea that some unknown action of disruptive discharge of this kind might be involved in the discrepancy. However that may be, the sensitiveness that Trouton had found to be possible in this experiment opens a path in another direction. Unless the FitzGerald-Lorentz change of dimensions is admitted, it follows from accepted electrical theory that an elongated electrified conductor carried through the æther must tend to set itself lengthways to the stream, being in unstable equilibrium when broadside on; and it turns out that the amount of the resulting second-order torque, when it is at a maximum half-way between these two positions of equilibrium, comes within the limits of detection, when the suspended body is a charged condenser such as Dr. Trouton employed. The second-order optical experiment of Michelson and Morley has hitherto stood alone—it has even been thought necessarily so on account of the unapproached precision of optical observations. But here is a feasible electrical experiment of mechanical type, which will confirm their result if its indications should also turn out to be negative. For this torque is the only means by which the energy could be conserved, as the system is rotated, if the dimensions of the system remain unaltered; it is not allowable* to suppose that the rotation of the charged body through a right angle may transform part of its electrical energy into other energy of some wholly internal type, which is recoverable in its previous form by the reverse operation. Even in the absence of experiment there are theoretical considerations that can be brought to bear. If the Fitz Gerald-Lorentz effect is non-existent, it is possible to imagine (*cf.* Nos. 107, 108 *infra*) an electrical arrangement which can convert the energy of the translatory motion of the Earth through the æther into mechanical work available between different mechanical systems on the Earth itself. The negation of this almost unlimited source of power, which in a certain sense might be classed as a "perpetual motion," carries with it the existence of the effect aforesaid. Moreover, if it were possible, even by a highly artificial arrangement, to transform energy of homogeneous translation of a body into energy of relative motion

* Cf. Heaviside, *Phil. Mag.* April, 1889; J. J. Thomson, 'Recent Researches in Electricity,' p. 18.

between its parts, which frictional action would ultimately degrade into heat, we are entitled to assert that in the infinitely varied vicissitudes of things this transformation would actually occur; and, as it is not completely reversible, energy of translatory motion would, in fact, thus suffer degradation, and there would be an æthereal resistance to the motion of bodies through space. The question of the conditions under which we are entitled to separate the mechanical and the thermal energies of a material system, and to discuss the transformations of each independently of the other, is fundamental for theoretical thermodynamics, and has, consequently, received attention.* The present considerations introduce a similar question in regard to æthereal energy.

This rapid review of the acquisition and settlement of a new region in physical science has been suggested by the recent departure from amongst us of one who, by general acclaim of his fellows, held the position of a leader in the enterprise. It has permitted us in a way to bring to a focus our estimate of FitzGerald's powers in the domain of physical thought. Looking back from our present position of vantage on his own published work, which represented but a small portion of his scientific activity, the main feature is its tentative and pioneering character and the continuous effort towards general views; we can see more in it than his contemporaries saw at the time, doubtless more even than he was clearly conscious of himself. The concentration in the exposition of the more original parts—while it has perhaps conspired with their fragmentary character to delay recognition of their width and grasp—keeps them still fresh and vivid after more elaborate memoirs have had their day. Although an accomplished mathematician, he preferred to reason in terms of direct images of the phenomena, and to reserve algebraic representations for purposes of calculation; he had early acquired the use of the quaternion analysis, and handled it with facility, and it was interesting to watch him getting to the bottom of a tangled scheme of physical relations by jotting down a few brief vector equations. The combined efforts of many men of genius are required to take full possession of a new territory; the subject must be for a time in the air; its ideas must be refined and polished by workers with various gifts, through the mutual attrition of apparently conflicting views, before the body of doctrine emerges in compact and intuitive form, ready to take its place

* Osborne Reynolds, *Phil. Trans.* 1895 A, Collected Papers, ii, p. 534; cf. also Larmor, 'Æther and Matter,' p. 280.

as common knowledge in the scientific inheritance of mankind. Here and there giants stand out, a Maxwell or a Hertz, and by some special achievement fix the attention of their contemporaries, or sometimes it may be only of their successors. But the greater number of workers are soon merged in their generation; they have helped to mould the ideas which will be the starting-point of the next, and their place may be not the less important though no definite priorities are claimed for them, or specific discoveries attached to their names.

One of the two Royal Medals was awarded to Fitz Gerald in the year 1899 by the Royal Society, for his contributions to theoretical physics, especially in the domain of optics and electrodynamics. The last preceding award to an Irish man of Science had been in 1881, to Rev. J. H. Jellett, afterwards FitzGerald's father-in-law, and also Provost of Trinity College, then known also as a weighty and incisive preacher and churchman, now remembered as a pioneer in the development of the relations of chemical equilibrium to the concentrations of the reacting constituents and the temperature,* as much as for his earlier profound investigations in mathematics and mathematical physics. Dr. Jellett was a skilful experimenter in the optics of rotation of the plane of polarization, and used this effect as a physical measure of the progress of chemical reaction. Community of tastes early brought Jellett and FitzGerald into close relationship. One who had unrivalled opportunities of appreciating them both remarks on "the great likeness in the two characters: the great simplicity, the directness of purpose, the utter absence of preaching but the living of the life that is the best, their great tenderness and love of children."

Fitz Gerald has died young, in the fiftieth year of his age, before any failure or loss of freshness had come over his mental powers. Important as have been his direct contributions to the intellectual progress of his generation, his influence over his fellows has been greater still. He was the ideal gentle knight, always more ready to afford what assistance he could to others than to proceed with his own speculations; even in his lifetime he has had his reward in becoming the trusted adviser and guide of some of the foremost scientific men of the age. His brilliant discourse to the Chemical Society of London in 1896 as the Helmholtz Memorial Lecture is remembered there as a classic. The impression produced by his loss may be judged

* 'Researches in Chemical Optics,' of dates 1860, 1863, 1873, in *Trans. R. I. Acad.* xxv. 1873, pp. 371–450.

from the affectionate appreciations collected recently in *Nature* (March 7) from Heaviside, Lodge, Ramsay, Perry and others of his scientific contemporaries.* Here it will suffice to insert some reminiscences received more recently from Lord Kelvin:

"My first meeting with Fitz Gerald was when we were both guests of Dr. Traill, of Trinity College, Dublin, at his country house near Bushmills, Co. Antrim, in the Easter vacation of 1883 or 1884, so far as I can remember, to visit the first electric railway of the British Empire, then recently opened between Portrush and the Giants' Causeway.

"Expecting to meet a mathematician of high order, a young and distinguished Fellow of Trinity College, I found much more than the great pleasure I anticipated. Mathematics and physics in abundance and of highly interesting quality we had, but in Fitz Gerald's personality there was a unique combination of which I never had experience before. His enthusiasm about water-power and electricity were delightful. We had many little and some considerable troubles in reconciling steam and electric power; and whenever a linesman or a dynamo-driver (a mechanic of species then new), or a fireman or an oiler, was in trouble, FitzGerald was there helping him as a comrade and acting practically as if to the manner born and educated. Our happy three days together on that occasion gave me an epitome of Fitz Gerald, living in an atmosphere of the highest scientific and intellectual quality, but always a comrade with every fellow-worker of however humble quality; just as I have known him ever since, and as he has been known for a quarter of a century to hosts of friends and brother workers in Ireland, England and Scotland.

"My scientific sympathy and alliance with him have greatly ripened during the last six or seven years over the undulatory theory of light and the æther theory of electricity and magnetism; and I feel keenly the scientific bereavement which his loss entailed on myself.

"As for his whole life, it seems to me that no one ever attained more nearly than Fitz Gerald to the chief end of man as defined by the shorter catechism of the Church of Scotland, 'to glorify God and to enjoy Him for ever.'" J. L.

* Cf. also Sir W. Huggins' Presidential Address to the Royal Society at the Anniversary Meeting, Nov. 30, 1901, published in the *R. S. Yearbook*, 1902, p. 184.

1

ON THE EQUATIONS OF EQUILIBRIUM OF AN ELASTIC SURFACE*

[From *Hermathena*, Vol. II, 1876.]

LAGRANGE has investigated the problem of the equilibrium of an elastic string in two ways, first supposing it to be perfectly flexible, and then on the hypothesis that it was like a spring.

He has only investigated the problem which corresponds to the first of these, in the case of an elastic surface; and I here propose to supply the omission.

It is not very easy to see at once what function to use as that which will best express the flexure of the surface, as we cannot use the total curvature which is unaltered by any bending of an inextensible surface. I therefore pitched upon $\left(\frac{1}{R} + \frac{1}{R'}\right) dS$ as most suitable: for if ds and ds' be elements of two lines of curvature, the bending round ds is $\frac{ds'}{R'}$ and this per unit of length of ds is $\frac{ds\,ds'}{R'}$, and similarly for ds'; so the total bending may be represented by $\left(\frac{1}{R} + \frac{1}{R'}\right) dS$, when dS is the element of surface $= ds\,ds'$.

* [The subject treated here is the statics of a curved sheet which is subjected to applied force (X, Y, Z) per unit area, and which reacts with a tension E uniform in all directions, opposing extension, and a torque F opposing pure bending only, and isotropic so as to be the same for bending in all planes. The torque is thus of special type. If we restrict the problem by imposing conservation of the strain-energy of the sheet, E must be a function of the areal extension, and F a function of l, as is in fact found later (p. 3) to be necessary when the applied forces have a potential. For the treatment of the general elastic problem in which the torque is not thus restricted, but under the physically important restriction of linear connexion between strain and stress, cf. Lord Rayleigh's *Theory of Sound*, Ed. 2, Vol. I, Chapters X, XA.]

We thus get for the equilibrium of a surface possessing both kinds of elasticity the following formula:—

$$\int\{\delta\Pi\, dS + E\delta\, ds + F\delta\left(\frac{1}{R}+\frac{1}{R'}\right) dS\} = 0,$$

calling E the elastic force of the surface, and F that of its flexure. Hence calling

$$\frac{1}{R}+\frac{1}{R'} = l = \frac{(1+q^2)r - 2pqs + (1+p^2)t}{U^3},$$

$$dS = U\,dx\,dy \quad \text{where} \quad U = \sqrt{1+p^2+q^2},$$

and

$$\delta\Pi = X\delta x + Y\delta y + Z\delta z,$$

we get, on integrating this out by parts according to the principles of the Calculus of Variations,

$$\begin{aligned}
0 = &\int\Big|_0'\Big[EU\delta x + E\frac{dU}{dp}\delta z + FU\left(\frac{dl}{dp}\delta z + \frac{dl}{dr}\delta p + \tfrac{1}{2}\frac{dl}{ds}\delta q\right)\Big]\, dy \\
&+\int\Big|_0'\Big[EU\delta y + E\frac{dU}{dq}\delta z + FU\left(\frac{dl}{dq}\delta z + \tfrac{1}{2}\frac{dl}{ds}\delta p + \frac{dl}{dt}\delta q\right)\Big]\, dx \\
&-\int\Big|_0'\Big[\frac{d}{dx}\left(FU\frac{dl}{dr}\right) + \tfrac{1}{2}\frac{dl}{dy}\left(FU\frac{dl}{ds}\right)\Big]\delta z\, dy \\
&-\int\Big|_0'\Big[\frac{d}{dy}\left(FU\frac{dl}{dt}\right) + \tfrac{1}{2}\frac{dl}{dx}\left(FU\frac{dl}{ds}\right)\Big]\delta z\, dx \\
&+\iint\left\{\begin{array}{l}
(X\delta x + Y\delta y + Z\delta z)\, U + E\left(\dfrac{dU}{dx}\delta x + \dfrac{dU}{dy}\delta y\right) \\
\qquad + FU\left(\dfrac{dl}{dx}\delta x + \dfrac{dl}{dy}\delta y\right) \\
-\dfrac{dEU}{dx}\delta x - \dfrac{dEU}{dy}\delta y \\
-\left[\dfrac{d}{dx}\left(E\dfrac{dU}{dp}\right) + \dfrac{d}{dy}\left(E\dfrac{dU}{dy}\right) + \dfrac{d}{dx}\left(FU\dfrac{dl}{dp}\right)\right. \\
+\dfrac{d}{dy}\left(FU\dfrac{dl}{dq}\right) - \dfrac{d^2}{dx^2}\left(FU\dfrac{dl}{dr}\right) - \dfrac{d^2}{dx\,dy}\left(FU\dfrac{dl}{ds}\right) \\
\qquad \left. - \dfrac{d^2}{dy^2}FU\dfrac{dl}{dt}\right]\delta z
\end{array}\right\} dx\,dy.
\end{aligned}$$

Hence as $\delta z = \delta' z - p\delta x - q\delta y$, calling P the coefficient of δz, we get the three following equations:—

$$UX + E\frac{dU}{dx} - \frac{dEU}{dx} + FU\frac{dl}{dx} + pP = 0,$$

$$UY + E\frac{dU}{dy} - \frac{dEU}{dy} + FU\frac{dl}{dy} + qP = 0,$$

$$UZ - P = 0.$$

From these, since $dz = p\,dx + q\,dy$, we get

$$Ud\Pi - UdE + FUdl = 0.$$

Therefore, as in all such cases, we get, assuming the forces to have a potential Π,

$$\Pi + a = E - \int Fdl.$$

From the equation, $UZ - P = 0$, we get, writing P at length:

$$UZ = \left\{ \begin{array}{c} \frac{d}{dx}\left(E\frac{dU}{dp}\right) + \frac{d}{dy}\left(E\frac{dU}{dq}\right) + \frac{d}{dx}\left(FU\frac{dl}{dp}\right) \\ + \frac{d}{dy}\left(FU\frac{dl}{dq}\right) \\ - \frac{d^2}{dx^2}\left(FU\frac{dl}{dr}\right) - \frac{d^2}{dxdy}\left(FU\frac{dl}{ds}\right) \\ - \frac{d^2}{dy^2}\left(FU\frac{dl}{dt}\right) \end{array} \right\}.$$

Now, from this, since

$$\frac{dU}{dp} = \frac{p}{U}, \quad \frac{dU}{dq} = \frac{q}{U};$$

$$\frac{dl}{dp} = 2\frac{pt - qs}{U^3} - \frac{3lp}{U^2}, \quad \frac{dl}{dq} = 2\frac{qr - ps}{U^3} - \frac{3lq}{U^2},$$

and since

$$l = \frac{d}{dx}\frac{p}{U} + \frac{d}{dq}\frac{q}{U},$$

and as these are total differentiations,

$$X + pZ = \frac{dE}{dx} - F\frac{dl}{dx}, \quad Y + qZ = \frac{dE}{dy} - F\frac{dl}{dy}, \text{ for } d\Pi = dE - Fdl,$$

and assuming $Z - pX - qY = UN$, N being the normal force, and since

$$\frac{dl}{dr} = \frac{1+q^2}{U^2}, \quad \frac{dl}{ds} = -\frac{2pq}{U^2}, \quad \frac{dl}{dt} = \frac{1+p^2}{U^2},$$

we get, on grouping the terms according to the order of the differentiations with regard to F,

$$N = l(E - Fl)$$

$$- F\left\{2l^2 + 2\left(\frac{p}{U}\frac{dl}{dx} + \frac{q}{U}\frac{dl}{dy}\right) - 2\left(\frac{d}{dx}\frac{pt-qs}{U^2} + \frac{d}{dy}\frac{qr-ps}{U^2}\right)\right.$$
$$\left. + \frac{d^2}{dx^2}\frac{1+q^2}{U^2} - 2\frac{d^2}{dxdy}\frac{pq}{U^2} + \frac{d^2}{dy^2}\frac{1+p^2}{U^2}\right\}$$

$$+ \frac{dF}{dx}\left\{2\frac{pt-qs}{U^2} - 3\frac{lp}{U} - 2\frac{d}{dx}\frac{1+q^2}{U^2} + 2\frac{d}{dy}\frac{pq}{U^2}\right\}$$

$$+ \frac{dF}{dy}\left\{2\frac{qr-ps}{U^2} - 3\frac{lq}{U} - 2\frac{d}{dy}\frac{1+p^2}{U^2} + 2\frac{d}{dx}\frac{pq}{U^2}\right\}$$

$$+ \frac{1+q^2}{U^2}\frac{d^2F}{dx^2} - \frac{2pq}{U^2}\frac{d^2F}{dxdy} + \frac{1+p^2}{U^2}\frac{d^2F}{dy^2}.$$

This can be very much simplified by differentiating these out, and the work will be comparatively short if the following method be adopted:—

Calling the third differentials $\frac{d^3z}{dx^3}$, &c., k, l, m, n, we see that

$$\frac{d}{dx}\frac{pt-qs}{U^2} = \frac{rt-s^2}{U^2} - \frac{2(pt-qs)(pr+qs)}{U^4} + \frac{pm-ql}{U^2},$$

$$\frac{d}{dy}\frac{qr-ps}{U^2} = \frac{rt-s^2}{U^2} - \frac{2(qr-ps)(ps+qt)}{U^4} + \frac{ql-pm}{U^2}.$$

Hence $\frac{d}{dx}\frac{pt-qs}{U^2} + \frac{d}{dy}\frac{qr-ps}{U^2} = 2\frac{rt-s^2}{U^4} = 2\Delta$, where $\Delta = \frac{1}{RR'}$

the total curvature.

It is also easy to see that

$$\frac{d}{dx}\frac{1+q^2}{U^2} - \frac{d}{dy}\frac{pq}{U^2} = -\frac{2lp}{U} + \frac{pt-qs}{U^2} = \Theta,$$

$$\frac{d}{dy}\frac{1+p^2}{U^2} - \frac{d}{dx}\frac{pq}{U^2} = -\frac{2lq}{U} + \frac{qr-ps}{U^2} = \Theta',$$

and the second differentials, in the coefficient of F, are $\frac{d\Theta}{dx} + \frac{d\Theta'}{dy}$.

But as $\frac{d}{dx}\frac{p}{U} + \frac{d}{dy}\frac{q}{U} = l$,

$$\frac{d\Theta}{dx} + \frac{d\Theta'}{dy} = -2l^2 - 2\left(\frac{p}{U}\frac{dl}{dx} + \frac{q}{U}\frac{dl}{dy}\right) + \frac{d}{dx}\frac{pt-qs}{U^2} + \frac{d}{dy}\frac{qr-ps}{U^2}.$$

Hence the coefficient of F is

$$-\frac{d}{dx}\frac{pt-qs}{U^2} - \frac{d}{dy}\frac{qr-ps}{U^2} = -2\frac{rt-s^2}{U^4} = -2\Delta.$$

Similarly the coefficient of $\frac{dF}{dx}$ is $\frac{lp}{U}$, and of $\frac{dF}{dy}$ is $\frac{lq}{U}$.

Hence we get at once

$$N = l(E - Fl) + 2F\Delta + l\left(\frac{p}{U}\frac{dF}{dx} + \frac{q}{U}\frac{dF}{dy}\right) + \frac{1+q^2}{U^2}\frac{d^2F}{dx^2} - \frac{2pq}{U^2}\frac{d^2F}{dxdy} + \frac{1+p^2}{U^2}\frac{d^2F}{dy^2},$$

which is rather an elegant expression for N.

2

ON THE EFFECTS OF MAGNETIZATION OF THE IRON IN A SHIP, ON THE COMPASS, WHEN THE SHIP HEELS

[From *Hermathena*, Vol. II, 1876.]

I ASSUME that the magnetization induced in any direction by the Earth is proportional to the cosine of the angle between that direction and that of the Earth's magnetism. Also that the moment of this induced magnetism on the needle in any direction is proportional to the cosine of the angle between these directions.

I divide the magnetization of the ship into fore and aft magnetization, beam magnetization, and vertical magnetization. It is to be observed that as the fore and aft magnetization is not symmetrical with respect to the compass, it will have a vertical disturbing moment, and the vertical one will similarly have a horizontal moment, and the coefficients of these, depending on the position of the compass in the ship, may be shown, as by Airy's method, to be the same.

I will only consider the easterly component of these disturbing forces, as that is the only one which deflects a compass as it is usually hung.

Suppose now that K is the moment of permanent vertical magnetism in the ship on the needle, and h is the angle of the ship's heeling, and A the westerly azimuth of the ship's head from the north. Then, assuming that the horizontal effect of this vertical magnetism has been already compensated, as is usually the case in ships, there only remains the easterly component $= -K \sin h \cos A$ due to the heeling.

The fore and aft magnetization is unaltered by the heeling. Calling δ the dip, and I the Earth's inductive force, the fore and aft induced magnetism will vary as $I \cos\delta \cos A$, and the easterly component of this on the needle will vary as $I \cos\delta \cos A \sin A$, and calling H the horizontal Earth's force $= I\cos\delta$, and $V = I \sin\delta$ the vertical force, and l the factor depending on the ship's shape, we get that the easterly component due to this cause is

$$= H\tfrac{1}{2}l \sin 2A.$$

The beam induced magnetism similarly varies as $I \cos IB$ where IB is the angle between the direction of the dip and the beam. This gives that it varies as $-I(\cos\delta \cos h \sin A + \sin h \sin\delta)$. Hence the easterly moment may be expressed as

$$= -Im\,(\cos\delta \cos h \sin A + \sin h \sin\delta)\cos A$$

$$= -H\tfrac{1}{2}m \sin 2A \cos h - Vm \sin h \cos A.$$

Similarly for the effect of the vertically induced magnetism we get two sources of deflection: 1°, due to the unsymmetrical position of this magnetization relatively to the compass; and 2°, due to the heeling of the ship making this, which is vertical to the ship's deck, have an easterly component. Let the moment of the first be n, and of the second r; then we get that the easterly component due to the first source is

$$Vn \cos h \sin A - nH \sin h \sin^2 A,$$

and due to the second source is

$$Vr \sin h \cos h \cos A - H\tfrac{1}{2}r \sin^2 h \sin 2A.$$

Besides these, the vertical component of the fore and aft magnetism will have the component due to heeling

$$Hn \sin h \cos^2 A.$$

Its moment is n, as well as the first of the vertical components, as I have already remarked.

Adding up all these easterly components we get

$$Y = H\tfrac{1}{2}l \sin 2A - H\tfrac{1}{2}m \cos h \sin 2A + Vn \cos h \sin A$$
$$- \sin h \{K \cos A + Vm \cos A + H\tfrac{1}{2}r \sin h \sin 2A$$
$$- Hn \cos 2A - Vr \cos h \cos A\}.$$

Ordering these according to the powers of $\sin h$ or $\sin \frac{1}{2}h$, and putting in $\frac{1}{2}(l - m) = p$, $m - r = s$, we get

$$Y = Hp \sin 2A + Vn \sin A$$
$$+ \sin h \{Hn \cos 2A - (K + Vs) \cos A\}$$
$$- \sin^2 \tfrac{1}{2}h \{(r \cos h - s) H \sin 2A + 2Vr \sin h \cos A$$
$$+ 2Vn \sin A\}.$$

Expanding to the first power of h, this gives the same result as Airy gets by his cumbrous process.

The principal value of these investigations is, of course, to get the form of the functions, as practically the constants have to be determined empirically in each particular ship.

3

ON THE ROTATION OF THE PLANE OF POLARIZATION OF LIGHT BY REFLECTION FROM THE POLE OF A MAGNET

[From the *Proceedings of the Royal Society*, No. 176, 1876.
See Nos. 7, 8 *infra*.]

At a meeting of the Dublin Scientific Club on Monday the 6th November, Professor Barrett gave the Club an account of Mr. Kerr's experiments on the rotation of the plane of polarization of a ray of light when reflected from the surface of the end of a magnet, to which additional interest was attached by the reading of a letter from Mr. Kerr to Professor Barrett giving an account of the mode of making and of the last results of his experiments. At the time I proposed trying whether any similar effects would be produced by reflection from the surface of a crystal of quartz cut perpendicularly to the axis, as I was led to think there might be, owing to the similarity of the rotatory polarization of quartz and of substances under magnetic action. Following out that clue, I obtained the following explanation of Mr. Kerr's experiment, and was enabled, through Professor Barrett's kindness in helping me to verify my recollections of Mr. Kerr's letter, to make sure that my theory explains the facts.

Faraday has shown, in the nineteenth series of his experimental researches, that a ray of plane-polarized light, when transmitted through any solid (diamagnetic ?) transparent medium under the action of a powerful magnet, has the plane of its polarization rotated in that direction in which a positive

current must circulate round the ray in order to produce a magnetic force in the same direction as that which actually exists in the medium. Verdet, however, discovered that in certain *ferro-magnetic* media (as, for instance, a strong solution of perchloride of iron in wood-spirit or ether) the rotation is in the opposite direction to the current which would produce the magnetic force.

Now Fresnel's explanation of the rotatory power of quartz has been applied by Professor Maxwell, in his "Electricity and Magnetism," vol. ii, p. 402, to explain the similar, though not identical, phenomenon of magnetic rotation of light. He there, in § 812, gives this explanation in the following words:—"A plane-polarized ray falls on the medium. This is equivalent to two circularly polarized rays, one right- and the other left-handed (as regards the observer). After passing through the medium the ray is still plane-polarized, but the plane of its polarization is turned, say, to the right (as regards the observer). Hence of the two circularly polarized rays, that which is right-handed must have had its phase accelerated with respect to the other during its passage through the medium. In other words, the right-handed ray has performed a greater number of vibrations, and therefore has a smaller wave-length within the medium than the left-handed ray which has the same periodic time." This is the same as saying that the velocity of the right-handed ray is less within the medium than the left-handed, or that the refractive index for right-handed rays is greater than for left-handed in a medium that rotates light to the right. Hence, from what Verdet has shown, it appears that, in a ferro-magnetic substance, *for a ray of light travelling from the south to the north pole, the magnetic action is such as to make the refractive index for right-handed circularly polarized rays less than for left-handed ones*; for in this case the plane of polarization is turned to the left, for it is a right-handed current that would produce the magnetic force.

By applying this to the case of light reflected from the south pole of a magnet, we get what I believe to be the true explana-

tion of Mr. Kerr's interesting experiment. In like manner as in the case of a transmitted ray, I consider the incident plane-polarized ray to be the resultant of two circularly polarized ones, one right- and the other left-handed. Now for the right-handed one, the refractive index at the surface of the south pole of the magnet, being a ferro-magnetic substance, is less than for the left-handed ray. Hence if each of the two circularly polarized rays be supposed to be the resultant of two plane-polarized rays, one polarized in the plane of incidence and the other at right angles to it, the intensities of these four rays being equal, it is evident that the intensities of the pair of reflected rays corresponding to the left-handed ray will be greater than the corresponding intensities of those due to the right-handed ray. Hence the two rays which were polarized perpendicularly to the plane of incidence, and which originally destroyed one another, will, after reflection, have a component in the direction of the vibration of the left-handed ray after reflection. Now, on account of the change of direction of the ray on reflection, this latter is towards the right. This is completely explained in M. Jamin's "Cours de Physique," vol. iii, part 2, p. 674, where he shows that a ray the azimuth of whose plane of polarization was originally towards the right is by reflection turned towards the left. Hence the result of reflection is to furnish two rays, one polarized in the plane of incidence, and the other at right angles to it. The phases of these rays will, in general, be different; for they differed by 90° before reflection, and except at the polarizing angle for iron, this difference of phase would not be completely destroyed, so that the resultant would generally be an elliptically polarized ray the direction of whose major axis would make a small angle towards the right with the plane of incidence; and at the polarizing angle for iron this ellipse would become a plane-polarized ray whose plane of polarization was turned towards the right, which I understand to be the direction in which Mr. Kerr observed it to be turned—although from some ambiguity as to the meaning of right and left rotations in a ray,

arising from not specifying whether it is relative to the direction in which the ray is going or in which it is observed, I am not quite sure whether I understand Mr. Kerr correctly. Also from the fact that there are exceptions* to the rule that rotations are positive for diamagnetic and negative for ferro-magnetic substances, neutral chromate of potash being diamagnetic yet producing a negative rotation, I should be rather inclined to deduce the direction of the rotation that would be produced, if iron were transparent, from Mr. Kerr's experiment.

It would be quite easy to deduce the difference of the refractive indices of iron for the two circularly polarized rays if we knew the amount by which the plane of polarization is turned; but it would be necessary to employ† MacCullagh's or Cauchy's formulæ for the intensities of the reflected rays; and these are so complicated that it is hardly worth while going through the calculations, as the effect Mr. Kerr has observed seems only barely observable.

Similar effects must, of course, occur in the cases of diamagnetic substances, organic solutions, and quartz; but the amounts in these cases would be entirely beyond the range of observation of our present instruments; for in quartz, for instance, the difference of the refractive indices of the two circularly polarized rays is only 0·00008.

Observations confirmatory of the foregoing Explanation.

[Received November 23, 1876.]

Since sending my explanation of Mr. Kerr's experiment I have made some experiments in confirmation of it. The instruments, with the exception of the electro-magnet which was kindly lent to me by Mr. Yeates, are the property of Trinity College, Dublin, and were placed at my disposal by Professor Leslie.

The electro-magnet I used is of the horseshoe pattern, with movable soft iron armatures, a face of one of these being well

* Unless, indeed, these are due to the nature of the solvent.

† [The waves of permanent type will be circularly polarized, and a new theory appropriate to them must be elaborated, as is done in Nos. 7, 8 *infra*.]

polished. The magnet was placed vertically, and the armatures were arranged on the poles so that the polished face was vertical, and a vertical edge of the other armature parallel and very close to this face. A folded piece of paper was inserted at the top between the edge and the face to prevent their being drawn together when the magnet was set in action. Two Nicol's prisms were so placed that a horizontal beam of light traversing one of them was reflected down the other by the polished face from that part of its surface which was opposite the edge.

A beam of sunlight was now transmitted through the apparatus and observed on emerging from the second Nicol. The following results were thus obtained:—When the light was polarized by the first Nicol, either in or perpendicularly to the plane of incidence, and when it had been extinguished by the analyzer, as soon as the electro-magnet was set in action the light immediately reappeared. On now slightly moving the analyzer the light could be partly extinguished; but no motion of the analyzer could make the field as black as it had been before the magnetism was excited, thus conclusively proving that what was produced was an elliptically polarized ray, as I had anticipated. When the light was reflected from a south pole the plane of polarization was rotated to the right of the observer, which is the direction of rotation assumed in my explanation.

I next covered a portion of the polished face with gold leaf, as Professor Barrett had suggested; and now the light reflected from this diamagnetic substance was unaffected by the magnetism, as I had also anticipated. I exhibited all these effects to Mr. Stoney, who entirely confirmed my observations.

[Received November 25, 1876.]

The angle of incidence in the experiments described above was about 60°. If the incidence were either perpendicular or grazing, the theory which I have proposed would lead to the conclusion that the angle between the major axis of elliptic

polarization and the original plane of polarization would vanish. If, accordingly, the observation can be made at a perpendicular incidence, and if the Nicol's prisms be so placed as to extinguish the light before magnetizing the iron, then, on exciting it, light ought to appear, as it does at oblique instances; but the field should not become darker on moving the analyzer.

I attribute great weight to the verification of my theory arising from the fact that the polarization of the reflected ray is found by experiment to be in general elliptic, and also from the fact that there is no appreciable effect when gold, a diamagnetic substance and therefore feeble, is substituted for iron.

Since communicating my paper, I learn, through Professor Stokes, that when Mr. Kerr's paper was read before Section A of the British Association, both he and Sir W. Thomson spoke of the possibility of connecting Mr. Kerr's result with a powerful double refraction of the same kind as the feeble double refraction shown by transparent substances under the influence of magnetism. It is a connexion of this kind which I have endeavoured to demonstrate.

4

SUGGESTIONS FOR AN EXPERIMENT TO DEMONSTRATE THE POLARIZED STATE OF THE GAS IN CROOKES'S LAYER

[From the *Scientific Proceedings* of the Royal Dublin Society. Read January 21, 1878. See No. 11 *infra*.]

I DESIRE to apologise to the Society for bringing before them only a proposed instead of a performed experiment, but my excuse is that it will probably be some time before I am able to perform the experiment myself, and as I desire to get credit for having at least proposed it, I take this opportunity of publishing it, and of giving my reasons for supposing it likely to be successful, by showing that the quantities involved are quite within the reach of our present methods of observing them.

I would first notice that, according to both Clausius' and Maxwell's theories of the conduction of heat in gases, the existence of a force like Crookes's* depends essentially upon the distribution of the velocities among the molecules, it being easily seen from either of their investigations (as is also evident from many other obvious considerations) that it is quite possible to imagine such a distribution of velocities among the molecules as that, though heat be propagated through the medium, yet the pressure in all directions shall be the same. Hence, any independent method of demonstrating a polarized state of the gas is of considerable importance; and the experiment I am about to propose is for the purpose of doing so.

[* This means the force by which the layer or cushion of vapour sustains a drop in the spheroidal state.]

When any homogeneous transparent substance is in a state of stress, its refractive index for light polarized in certain planes is different from that for light polarized in other planes, and consequently a ray of plane polarized light, when passed through such a medium in certain directions, emerges elliptically polarized. If the gas in a Crookes's Layer be in the state of stress that theory indicates, it ought to behave similarly, and a plane polarized beam when transmitted along it should emerge elliptically polarized. Now, the amount of elliptic polarization, we may expect, can be calculated in the following manner:—I have estimated by a series of unfortunately rather rough experiments that when a red-hot ball is plunged into cold water to a depth of half a centimetre, the thickness of the Crookes's Layer formed is about a quarter of a millimetre. This is, of course, only a rough approximation, but it will give us results which determine with what order of quantity we are dealing. Hence the excess of pressure in the vertical over that in the horizontal direction that I measured was half a gramme per sq. centimetre, which is about the ·0005 of the atmospheric pressure. Now, I will make an assumption which is, however, only partially true, but as one object of the experiment is to determine to what extent it is so it is legitimate provisionally, and it is that we may treat the strain in the gas as due entirely to a difference of density in different directions. That we may do so, to some extent at least, is manifest, for, according to theory, the number of molecules moving in the direction of the strain is greater than the number moving in other directions. To what extent this is true could only be determined either by elaborate theoretical investigations into the state of the gas or else by experiments such as I am proposing. Assuming then the strain to be wholly due to a difference of density we can proceed as follows:—The law connecting the refractive indices of a gas at different densities is $\frac{\mu - 1}{d} = \frac{\mu' - 1}{d'}$, so that in air when $\mu = 1{\cdot}0002940$, and in the case we are considering where $\frac{d'}{d} = 1{\cdot}0005$, we have $\mu' = 1{\cdot}0002941$. As there are 100,000 vibrations of light per

five centimetres *in vacuo*, there will be 100029·40 when the density is μ, and 100029·41 when it is μ', and consequently a difference of phase of ·01 of a wave-length will be introduced per five centimetres, or a twentieth of a wave-length in 25 centimetres. Now, as the intensity of light in the analyzer depends upon the square of the sine of the difference of phase, this will give the intensity as the square of the sine of 9°, which is ·02 or $\frac{1}{50}$th. Hence I conclude that if a ray of plane polarized light be transmitted through 25 centimetres of a Crookes's Layer between two surfaces, one of which is red-hot, and the other below the temperature of boiling water, and with an interval between them of a quarter of a millimetre, then one-fiftieth of the light will be restored in an analyzer which was so turned as to extinguish the beam before transmission between the plates. Such an effect could be easily observed, but as it is very unlikely that the whole of the strain in a Crookes's Layer is due to a difference of density of the gas in different directions, it is very unlikely that so great an effect would be produced. One object, however, in trying the experiment would be to determine to what extent the strain is due to a difference of densities.

5

ON THE MECHANICAL THEORY OF CROOKES'S FORCE

[From the *Scientific Transactions* of the Royal Dublin Society, 1878. Read March 18, 1878.]

WHEN two surfaces at different temperatures are in presence of one another with a gas between them, there exists a force tending to separate them. The assumption of this force explains a very great number of phenomena, including the motion of the arms of Mr. Crookes's radiometers and the so-called spheroidal state of liquids. That this force was due to some sort of unequal stress in the gas between the two surfaces was pointed out by Mr. Stoney, in the Phil. Mag., March and April, 1876, where he attempted to show that such a state of stress would arise. An attempt to explain the motion of the arms of a radiometer had been made previously by Professor O. Reynolds,* but his conclusion, that it was principally due to evaporation and condensation, is manifestly inadequate to explain a continuous action, such as that in a radiometer; and the method by which he tried to show that a surface when communicating heat to gas is subject to an increased pressure, is open to the overwhelming objection, that this increased pressure would be almost instantaneously transmitted to all parts of the enclosed gas, and so could not possibly be the source of such a force as would explain the motion of the arms of a radiometer.

In amplification of a letter I wrote to *Nature* on the 17th of December, 1877, and which was published on the 4th of January, 1878, I now intend to prove that such a state of stress

[* See, however, Introduction.]

as Mr. Stoney's theory requires, would exist under the assumed conditions. My letter contains a proposed application of Clausius' investigation for finding the conducting power of a gas, as published in the Phil. Mag., vol. 23, 4th Ser. Mr. Stoney, in a paper read before the Royal Dublin Society, on Monday, the 18th of February, 1878, has obtained results somewhat like those obtained by my method, by applying a method similar to one he originally employed.

I may first observe, that the only way in which a state of other than uniform stress can exist in a gas, is by the distribution of the mean velocities and number of molecules being different in different directions, or, as Mr. Stoney has called it, by the gas being polarized. That the distribution is not uniform when heat is being conducted through a gas, has been pointed out long ago by both Clausius and Maxwell, and what is required is to show that the distribution will then be such as to develop a force like Crookes's.

Following the method adopted by Clausius in his Paper already referred to, I assume that the mean velocity of a molecule is a function of its direction of motion, and that the number of molecules in the unit volume, moving in a given direction, is also a function of that direction. If, then, we define the direction by means of μ, the cosine of the angle it makes with a given direction, and ϕ, the angle the plane of these two directions make with a fixed plane through the given direction, we may evidently assume,

$$v = v_0 f(\mu, \phi), \qquad n = n_0 \mathrm{F}(\mu, \phi),$$

where v and n are the mean velocity and number of molecules moving in this direction, and v_0 and n_0 are certain given values of v and n when f and F are unity. Now we may evidently in addition take $n_0 = \frac{\mathrm{N}}{4\pi} d\mu d\phi$ where N is the total number of molecules per unit volume, so that we have generally

$$n = \frac{\mathrm{N}}{4\pi} \mathrm{F}(\mu, \phi)\, d\mu\, d\phi.$$

The quantities I intend to calculate are, the number of

molecules carried through the unit area in any direction, the total momentum carried through the same, and the quantity of energy carried through it. The number of molecules going in one direction through the unit area must evidently be equal to that of those going in the opposite direction, if there are no gaseous currents going on; and, even if present, their existence is evidently beside the question in hand. Hence, if we sum the number of molecules passing the unit area, taking those that go in opposite direction through it with opposite signs, the sum must vanish. I shall calculate the numbers in three cases of unit areas; 1st, perpendicular to the line from which μ is measured, or X; 2nd, parallel to the plane from which ϕ is measured, *i.e.* perp. to Y; and 3rd, for the case of a unit area perpendicular to these two, *i.e.* perp. to Z. The number of molecules going in the direction (μ, ϕ), that pass through the first of these per unit of time, is evidently $= nv\mu$; and it is likewise evident that the number going in the opposite direction will have an opposite sign, so that we have the sum of all such zero. Similarly for the other two planes the numbers are

$$nv\sqrt{1-\mu^2}\sin\phi \text{ and } nv\sqrt{1-\mu^2}\cos\phi.$$

So that we get

$$0 = \Sigma nv\mu = \Sigma nv\sqrt{1-\mu^2}\sin\phi = \Sigma nv\sqrt{1-\mu^2}\cos\phi.$$

The momentum carried through the first of these unit areas per unit of time, by molecules moving in the direction μ, ϕ is $= Mnv^2\mu^2$ if M be the mass of each molecule; and as it does not change sign with μ, we see that the sum of all such will represent the normal pressure per unit area, at the given place. We can similarly get the normal pressures on the other two unit areas, and calling them P_{xx}, P_{yy}, and P_{zz}, we obtain

$$P_{xx} = M\Sigma nv^2\mu^2,$$

$$P_{yy} = M\Sigma nv^2(1-\mu^2)\sin^2\phi,$$

$$P_{zz} = M\Sigma nv^2(1-\mu^2)\cos^2\phi.$$

Proceeding similarly we can get the tangential pressures on these areas, and we easily see that they are

$$P_{yz} = P_{zy} = M\Sigma nv^3(1-\mu^2)\sin\phi\cos\phi,$$

$$P_{zx} = P_{xz} = M\Sigma nv^3\mu\sqrt{1-\mu^2}\cos\phi,$$

$$P_{xy} = P_{yx} = M\Sigma nv^3\mu\sqrt{1-\mu^2}\sin\phi.$$

If now we proceed to calculate the energies carried across these areas per unit time, we get $k n v^3 \mu$ as that carried across the 1st area by molecules moving in the direction μ, ϕ, where k is the coefficient by which the energy of translation must be multiplied in order to obtain the total energy; calling the quantities of energy Q_x, Q_y, and Q_z, we thus get

$$Q_x = Mk\Sigma nv^3\mu, \qquad Q_y = Mk\Sigma nv^3\sqrt{1-\mu^2}\sin\phi,$$

$$Q_z = Mk\Sigma nv^3\sqrt{1-\mu^2}\cos\phi.$$

In order to be able to perform these summations, it is necessary to know the mean values of nv, nv^2, and nv^3 in terms of μ and ϕ, and I shall, in the first place, merely assume that they can be expanded in a series of spherical harmonies, thus:

$$\overline{nv} = \frac{Nv_0}{4\pi}(A_0 + A_1 + A_2 + \dots\dots)\,d\mu d\phi,$$

$$\overline{nv^2} = \frac{Nv_0^2}{4\pi}(B_0 + B_1 + B_2 + \dots\dots)\,d\mu d\phi,$$

$$\overline{nv^3} = \frac{Nv_0^3}{4\pi}(C_0 + C_1 + C_2 + \dots\dots)\,d\mu d\phi.$$

The effect of this is to obtain our former results under the following simplified forms. Our first series of equations gives $A_1 = 0$, and as A_1 must be of the form

$$A_1 = a_1\mu + a_2\sqrt{1-\mu^2}\sin\phi + a_3\sqrt{1-\mu^2}\cos\phi,$$

we get $$a_1 = a_2 = a_3 = 0.$$

The second system of equations gives

$$P_{xx} = \frac{MNv_0^2}{4\pi} \iint (B_0 + B_2)\mu^2 d\mu d\phi,$$

$$P_{yy} = \frac{MNv_0^2}{4\pi} \iint (B_0 + B_2)(1-\mu^2)\sin^2\phi \, d\mu d\phi,$$

$$P_{zz} = \frac{MNv_0^2}{4\pi} \iint (B_0 + B_2)(1-\mu^2)\cos^2\phi \, d\mu d\phi,$$

$$P_{yz} = P_{zy} = \frac{MNv_0^2}{4\pi} \iint B_2(1-\mu^2)\sin\phi\cos\phi \, d\mu d\phi,$$

$$P_{zx} = P_{xz} = \frac{MNv_0^2}{4\pi} \iint B_2\mu\sqrt{1-\mu^2}\cos\phi \, d\mu d\phi,$$

$$P_{xy} = P_{yx} = \frac{MNv_0^2}{4\pi} \iint B_2\mu\sqrt{1-\mu^2}\sin\phi \, d\mu d\phi.$$

If now we assume

$$B_2 = b_1(\mu^2 - \tfrac{1}{3}) + b_2(1-\mu^2)\cos 2\phi + b_3(1-\mu^2)\sin\phi\cos\phi + b_4\mu\sqrt{1-\mu^2}\cos\phi + b_5\mu\sqrt{1-\mu^2}\sin\phi,$$

as it must be of this form, we get on putting our other quantities into the forms of spherical harmonics

$$P_{xx} = \frac{MNv_0^2}{3}\left(B_0 + \frac{4}{15}b_1\right),$$

$$P_{yy} = \frac{1}{3}MNv_0^2\left(B_0 - \frac{2}{15}b_1 - \frac{2}{5}b_2\right),$$

$$P_{zz} = \frac{1}{3}MNv_0^2\left(B_0 - \frac{2}{15}b_1 + \frac{2}{5}b_2\right),$$

$$P_{yz} = \frac{1}{15}MNv_0^2 b_3 = P_{zy}$$

$$P_{zx} = \frac{1}{15}MNv_0^2 b_4 = P_{xz}$$

$$P_{xy} = \frac{1}{15}MNv_0^2 b_5 = P_{yx}.$$

Similarly for the quantities of energy transferred we get

$$Q_x = \frac{MNv_0^3}{4\pi} k \iint C_1 \mu d\mu d\phi,$$

$$Q_y = \frac{MNv_0^3}{4\pi} k \iint C_1 \sqrt{1-\mu^2} \sin\phi d\mu d\phi,$$

$$Q_z = \frac{MNv_0^3}{4\pi} k \iint C_1 \sqrt{1-\mu^2} \cos\phi \, d\mu d\phi,$$

so that if we assume, as we evidently may,

$$C_1 = c_1\mu + c_2\sqrt{1-\mu^2} \sin\phi + c_3\sqrt{1-\mu^2} \cos\phi,$$

we get

$$Q_x = \frac{k}{3} MNv_0^3 c_1, \quad Q_y = \frac{k}{3} MNv_0^3 c_2,$$

$$Q_z = \frac{k}{3} MNv_0^3 c_3.$$

Even in this most general form we can see that there will, in general, be a difference of pressure in different directions. For it is evident that the pressures in the three directions cannot be equal unless b_1 and b_2 both vanish, which will not, in general, be the case. Without a knowledge of the nature of the distribution of the velocities and numbers of molecules moving in the different directions, it would be impossible to calculate the values of b_1, b_2, b_3, b_4, and b_5; but I think we can see that they will in part at least vary as the square of the quantity of heat passing. This can be seen from the following considerations. No matter what the distribution of the velocities and number of molecules moving in the different directions may be, it is plain that terms occurring in the coefficients of $\sqrt{1-\mu^2} \sin\phi$ and $\sqrt{1-\mu^2} \cos\phi$, *i.e.* in the spherical harmonics of the 1st order in u and v, will occur in the terms of the same order in nv, nv^2, and nv^3, and that linearly, while these same terms will occur squared in the spherical harmonics of the 2nd order in nv, nv^2, and nv^3. Hence we see that terms occurring linearly in the spherical harmonics of the 1st order in nv^3 will occur as squares in the spherical harmonic of the 2nd order in nv^3, so that b_1, b_2 will contain c_1, c_2, and c_3 in

the second degree, *i. e.* will contain terms varying as the squares of the quantities of heat passing. It is also to be observed that terms occurring in the spherical harmonics of the second order can never come into those of the first, except as products with terms belonging to spherical harmonics of the third order, so that a hypothetical distribution which gave correct values for the quantities of heat passing might very well be quite inadequate as a means of calculating the difference of pressure in different directions. This remark is of importance when we come to consider the results of Clausius' hypothesis, and was suggested to me by Mr. Stoney in conversation.

As an example of what I am insisting upon, we may take two opposite extreme cases. First, the case of B_2 vanishing, and secondly the case of C_1 doing so. In the first case there would be a distribution of velocities and numbers such that, though heat would be conducted across the layer, nevertheless there would be no resultant inequality of stress, while in the second case, though no heat would be conducted, yet there would be inequality of stresses. It seems, however, certain that neither of these extreme cases can exist as a permanent distribution in gases. Before calculating the values of these quantities upon particular hypothetical distributions, it may be well to see what they are in the simple case of two parallel planes each at a uniform temperature.

In this case it is evident from symmetry that if we take X normal to the planes we must have all our equations independent of ϕ, as the effect is evidently symmetrical with regard to X. Then we get

$$b_2 = b_3 = b_4 = b_6 = 0 = c_2 = c_3,$$

and there are no tangential forces, while all the heat is transferred in the direction X, and our pressures become

$$P_{xx} = \frac{1}{3} MN v_0^2 \left(B_0 + \frac{4}{15} b_1 \right),$$

$$P_{yy} = P_{zz} = \frac{1}{3} MN v_0^2 \left(B_0 - \frac{2}{15} b_1 \right),$$

while the heat transferred is

$$Q_x = \frac{k}{3} MN v_0^3 c_1.$$

The excess of pressure in X over that in the normal directions is

$$P_{xx} - P_{yy} = \frac{2}{15} MN v_0^3 b_1 = K,$$

and this has been called Crookes's force.

That it depends wholly upon b_1 can be seen by the following simple method mentioned to me by Mr. Stoney.

Our expressions for P_{xx} and P_{yy} are

$$P_{xx} = M\Sigma n v^2 \mu^2, \quad P_{yy} = M\Sigma n v^2 (1 - \mu^2) \sin^2\phi;$$

so that calling

$$n = \frac{N}{4\pi} I \, d\mu \, d\phi,$$

where I depends upon the distribution of numbers only, we can write the pressures

$$P_{xx} = \frac{MN}{4\pi} \iint I v^2 \mu^2 d\mu d\phi, \quad P_{yy} = \frac{MN}{4\pi} \iint I v^2 (1 - \mu^2) \sin^2\phi d\mu d\phi.$$

We can integrate them with respect to ϕ, for we know that Iv^2 is independent of ϕ in the case we are considering;

$$\therefore \; P_{xx} = \tfrac{1}{2} MN \int I v^2 \mu^2 d\mu, \quad P_{yy} = \tfrac{1}{4} MN \int I v^2 (1 - \mu^2) d\mu;$$

$$\therefore \; P_{xx} - P_{yy} = K = \tfrac{3}{4} MN \int I v^2 (\mu^2 - \tfrac{1}{3}) d\mu.$$

So that if Iv^2 be expanded in spherical harmonics, K depends only upon the spherical harmonic of the second order. Similarly if Iv^3 be similarly expanded, it is easy to see that

$$Q_x = \tfrac{1}{2} MNk \int I v^3 \mu d\mu$$

can only depend upon the spherical harmonic of the first order in Iv^3.

If now we turn to particular hypotheses as to the character of the distribution of velocities and numbers, the first that claims our attention is Clausius'. He starts from the assumption

that the distribution of velocities among the molecules that have just encountered one another in any given layer, may be perfectly represented by supposing a small constant velocity in the direction of the transference of heat to be superposed upon a uniform distribution. This is the same as supposing that these velocities in various directions may be represented by the radii drawn to the surface of a sphere from a point slightly displaced from its centre. It is worthy of remark, in connexion with what I mentioned before with reference to the way the quantities in the various spherical harmonics are related to one another, that supposing the sphere to be an ellipsoid of even great ellipticity would not have affected his results, for it is easy to show that the ellipticity of an ellipsoid of revolution only enters into the spherical harmonics of the second and higher orders, so that it would not enter into the equation giving the quantity of heat except when multiplied by terms of at least the order of the quantity of heat. Thus even though the square of the ellipticity were of the order of the displacement from the centre of the point from which the radii representing the velocities are drawn, nevertheless that would at most only have introduced terms depending upon the product of these two, which would not have materially affected his results. Hence we see that Clausius' success in calculating the quantity of heat conducted is no proof that his hypothesis is by any means a sufficient representation of the actual distribution for the purpose of calculating the resultant stresses, and that it is not, is proved by calculating what the Crookes's force would be upon his hypothesis. If this be done with the help of the quantities he gives in his note (see Phil. Mag., vol. 23, 4th Ser., p. 526), we get

$$K = \frac{1.8}{k^2} \frac{\rho_0 T_0}{P_0} \frac{Q^2}{PT},$$

and the pressures deduced from this formula are very much smaller than those observed, so that it seems certain that the hypothetical distribution Clausius assumed is not at all adequate to represent the actual one. The pressures obtained by this

formula are so insignificant that it is not worth while giving the details of the method by which it is deduced. That Clausius' hypothesis is by no means adequate can also be seen by the consideration that it is only after the Clausian laws for the conduction of heat have ceased to apply, owing to the rarefaction of the gas, that Crookes's force becomes remarkable, as well as by considering what the distribution tends towards in this case, when the number of molecules is small compared with the distance between the heater and cooler, as has been done by Mr. Stoney in his Paper read before this Society at its last meeting. He shows, as is also pretty evident, that the distribution tends towards one which could be represented by two unopposing streams of molecules moving one towards the heater and the other towards the cooler. With such a distribution the laws of conduction of heat would of course differ somewhat from those deduced from Clausius' distribution.

I shall now calculate the result upon an arbitrarily assumed distribution, which, however, probably represents the actual one more nearly than Clausius'. I shall assume that the distribution of velocities can be represented by the formula

$$\begin{aligned} v = v_0(1 &+ \alpha\cos\theta + \beta\sin\theta\sin\phi + \gamma\sin\theta\cos\phi \\ &+ a\cos^2\theta + b\sin^2\theta\sin^2\phi + c\sin^2\theta\cos^2\phi \\ &+ 2f\sin^2\theta\sin\phi\cos\phi + 2g\sin\theta\cos\theta\cos\phi \\ &+ 2h\sin\theta\cos\theta\sin\phi), \end{aligned}$$

where $\cos\theta = \mu$.

This is equivalent to saying that it is represented very nearly by the radii drawn to the surface of a slightly elliptical ellipsoid from a point near its centre. I shall assume that $\alpha\ \beta\ \gamma\ a\ b\ c\ f\ g\ h$ are all quantities whose squares and products may be neglected. For the number of molecules moving in the given direction θ, ϕ, I shall assume that it varies inversely as the velocity of the molecules moving in that direction, so that $nv = Nv_0$. This evidently satisfies the condition $A_1 = 0$. By these assumptions we obtain approximately

$$nv^2 = Nv_0v \quad \text{and} \quad nv^3 = Nv_0v^2,$$

and hence

$$nv^2 = Nv_0^2 \left\{ \begin{array}{l} (1 + a\mu + \beta\sqrt{1-\mu^2}\sin\phi + \gamma\sqrt{1-\mu^2}\cos\phi \\ \quad + a\mu^2 + b(1-\mu^2)\sin^2\phi + c(1-\mu^2)\cos^2\phi \\ \quad + 2f\sqrt{1-\mu^2}\sin\phi\cos\phi + 2g\mu\sqrt{1-\mu^2}\cos\phi \\ \quad + 2h\mu\sqrt{1-\mu^2}\sin\phi) \end{array} \right\},$$

or turning it into the form of a series of spherical harmonics

$$nv^2 = Nv_0^2 \left\{ \begin{array}{l} 1 + \frac{1}{3}(a+b+c) + (a - \frac{1}{2}b + c)(\mu^2 - \frac{1}{3}) \\ \quad + \frac{1}{2}(c-b)(1-\mu^2)\cos 2\phi + 2f\sqrt{1-\mu^2}\sin\phi\cos\phi \\ \quad + 2g\mu\sqrt{1-\mu^2}\cos\phi + 2h\mu\sqrt{1-\mu^2}\sin\phi \\ \quad + a\mu + \beta\sqrt{1-\mu^2}\sin\phi + \gamma\sqrt{1-\mu^2}\cos\phi \end{array} \right\},$$

from which we see that

$$b_1 = a - \tfrac{1}{2}(b+c), \quad b_2 = \tfrac{1}{2}(c-b), \quad b_3 = 2f, \quad b_4 = 2g, \quad b_5 = 2h.$$

We may evidently include the $\frac{1}{3}(a+b+c)$ in the mean value of Nv_0, and take $B_0 = 1$, so that calling $MN = \rho$, the density of the gas, our pressures become

$$P_{xx} = \tfrac{1}{3}\rho v_0^2 [1 + \tfrac{4}{15}\{a - \tfrac{1}{2}(b+c)\}],$$

$$P_{yy} = \tfrac{1}{3}\rho v_0^2 [1 + \tfrac{4}{15}\{b - \tfrac{1}{2}(c+a)\}],$$

$$P_{zz} = \tfrac{1}{3}\rho v_0^2 [1 + \tfrac{4}{15}\{c - \tfrac{1}{2}(a+b)\}],$$

$$P_{yz} = \tfrac{2}{15}\rho v_0^2 f = P_{zy},$$

$$P_{zx} = \tfrac{2}{15}\rho v_0^2 g = P_{xz},$$

$$P_{xy} = \tfrac{2}{15}\rho v_0^2 h = P_{yx}.$$

Similarly from $nv^3 = Nv_0 v^3$ we can get

$$c_1 = 2a, \qquad c_2 = 2\beta, \qquad c_3 = 2\gamma,$$

and hence

$$Q_x = \tfrac{2}{3}k\rho v_0^3 a, \qquad Q_y = \tfrac{2}{3}k\rho v_0^3 \beta, \qquad Q_z = \tfrac{2}{3}k\rho v_0^3 \gamma.$$

The normal pressures may also be put into the form

$$P_{xx} = \tfrac{1}{3}\rho v_0^2\{1 + \tfrac{1}{15}(a + b + c) + \tfrac{1}{5}(a - b - c)\},$$

$$P_{yy} = \tfrac{1}{3}\rho v_0^2\{1 + \tfrac{1}{15}(a + b + c) + \tfrac{1}{5}(b - c - a)\},$$

$$P_{zz} = \tfrac{1}{3}\rho v_0^2\{1 + \tfrac{1}{15}(a + b + c) + \tfrac{1}{5}(c - a - b)\}.$$

So that the state of stress is a uniform pressure, and superposed upon it a system of pressures represented by the equations

$$p_{xx} = \tfrac{1}{3}\rho v_0^2 \tfrac{1}{5}(a - b - c), \qquad p_{yz} = \tfrac{1}{3}\rho v_0^2 \tfrac{2}{5} f = p_{zy},$$

$$p_{yy} = \tfrac{1}{3}\rho v_0^2 \tfrac{1}{5}(b - c - a), \qquad p_{zx} = \tfrac{1}{3}\rho v_0^2 \tfrac{2}{5} g = p_{xz},$$

$$p_{zz} = \tfrac{1}{3}\rho v_0^2 \tfrac{1}{5}(c - a - b), \qquad p_{xy} = \tfrac{1}{3}\rho v_0^2 \tfrac{2}{5} h = p_{yx}.$$

Now it is remarkable that if $ax^2 + by^2 + cz + 2yz + 2gzx + 2hxy = (lx + my + nz)^2$ we should have expressions for these additional unequal pressures, the same as Prof. Clerk Maxwell gives (see his "Electricity and Magnetism," vol. i, p. 129, and vol ii, p. 256) as expressing that state of stress in the ether which produces electrical phenomena. In order to make them identical all that is necessary is to put

$$X = l\sqrt{\frac{8\pi}{15}\rho v_0^2}, \qquad Y = m\sqrt{\frac{8\pi}{15}\rho v_0^2}, \qquad Z = n\sqrt{\frac{8\pi}{15}\rho v_0^2},$$

so that the resultant unequal pressures in the gas may be represented by a pressure $p = \dfrac{R^2}{8\pi}$ when $R^2 = X^2 + Y^2 + Z^2$ in the direction given by

$$\mu : \sqrt{1-\mu^2}\sin\phi : \sqrt{1-\mu^2}\cos\phi :: X : Y : Z :: l : m : n,$$

and an equal diminished pressure in every direction at right angles to this line. Double this pressure will be the Crookes's force, which is consequently in this case

$$K = \tfrac{2}{3}\rho v_0^2 \tfrac{1}{5}(l^2 + m^2 + n^2),$$

and it is in the direction whose direction cosines are proportional to $l : m : n$, so that if we put

$$l = \nu\mu, \qquad m = \nu\sqrt{1-\mu^2}\sin\phi, \qquad n = \nu\sqrt{1-\mu^2}\cos\phi,$$

$$K = \tfrac{2}{15}\rho v_0^2 \nu^2.$$

The direction cosines of the line of transference of heat are evidently $\alpha : \beta : \gamma$; and so far there is no reason why these two lines should coincide, although, of course, in most cases, they probably differ but little in direction.

The only other distribution I shall consider is one suggested by Mr. Stoney's investigation (Trans. R. D. S., 1878, p. 39) of the nature of the distribution of the velocities in the gas between two large parallel surfaces at uniform unequal temperatures. He has shown that it tends towards a distribution which would be represented by two streams of unpolarized gas moving in opposite directions across the layer. Now the actual distribution is never exactly this, and possibly, as he has mentioned, departs in various degrees from it as you pass across the layer. If, however, we assume the distribution to be the same all the way across, and that consequently the mean temperature of each stream is that due to the surface it is leaving, we can calculate the resultant pressures.

If v_1 and v_2 be the mean velocities of the molecules in each stream, respectively, relatively to the centres of mass of the molecules, and if u_1 and u_2 be the velocities of the streams, *i. e.* of their centres of mass, and ρ_1 and ρ_2 their densities, the pressure upon a fixed plane normal to the direction of the streams is

$$P = \tfrac{1}{3}\rho_1 v_1^2 + \tfrac{1}{3}\rho_2 v_2^2 + \rho_1 u_1^2 + \rho_2 u_2^2,$$

while the pressure sideways is

$$p = \tfrac{1}{3}\rho_1 v_1^2 + \tfrac{1}{3}\rho_2 v_2^2,$$

so that the Crookes's pressure in this case is

$$K = P - p = \rho_1 u_1^2 + \rho_2 u_2^2.$$

In order that there be no accumulation of gas at either surface, we must evidently have

$$\rho_1 u_1 = \rho_2 u_2.$$

If V_1^2 and V_2^2 be the total mean squares of the velocities of agitation $V_1^2 = v_1^2 + u_1^2$, $V_2^2 = v_2^2 + u_2^2$, and the quantity of heat transferred is

$$Q = k(\rho_1 V_1^2 u_1 - \rho_2 V_2^2 u_2),$$

k being as before the coefficient by which the *vis viva* of translation has to be multiplied in order to get the total energy of the gas.

From these we easily obtain

$$K = \rho_1 u_1 (u_1 + u_2),$$

$$Q = k\rho_1 u_1 (V_1^2 - V_2^2);$$

$$\therefore \quad Q = kK \frac{V_1^2 - V_2^2}{u_1 + u_2}.$$

We have besides $\rho_1 + \rho_2 = \rho$, where ρ is the density of the gas. Hence there are six equations between the six unknowns

$$\rho_1, \rho_2, v_1, v_2, u_1, u_2,$$

and in order to eliminate them and obtain an equation between K and Q, it is necessary to make one further assumption. I assume then that $u_1 = \lambda v_1$ and $u_2 = \lambda v_2$, so that $V_1^2 = (\lambda^2 + 1) u_1^2$ and $V_2^2 = (\lambda^2 + 1) u_2^2$. I assume this, because if the streams did not interfere with one another at all, we should have $u_1^2 = \frac{1}{6} V_1^2$, so that if $\lambda^2 + 1 = a^2$ we should have $a^2 = 6$ and $a = 2{\cdot}5$ *q.p.*

Our equations then become

$$V_1^2 - V_2^2 = a^2 (u_1^2 - u_2^2);$$

$$\therefore \quad Q = kKa^2 (u_1 - u_2);$$

from these we can eliminate $u_1 u_2 \rho_1 \rho_2$, and putting $V_1^2 - V_2^2 = X^2$, we get

$$Q^4 + 4 \frac{a^4 k^2}{\rho} K^3 Q^2 - a^4 k^4 X^4 K^4 = 0,$$

which is a quadratic for Q^2 or a biquadratic for K.

Solving for Q, we get

$$Q = \frac{Kka}{\sqrt{\rho}} \left\{ \sqrt{X^4 \rho^2 + 4a^4 K^2} - 2a^2 K \right\}^{\frac{1}{2}},$$

as evidently the other solutions are inadmissible.

From this we may get an approximate value for K in terms of Q; for unless a be very large or the density or difference of

temperature very small, $X^2\rho$ is much greater than $2a^2K$. For instance, if V_1 and V_2 correspond to a difference of 10° C.,

$$V_1 = 48500\sqrt{\frac{T_1}{273}}, \qquad V_2 = 48500\sqrt{\frac{T_2}{273}},$$

and consequently

$$X^2 = \frac{(48500)^2}{27{\cdot}3}; \qquad \therefore \quad X = 9700,$$

while $\rho = \frac{1}{800}$ for air at atmospheric pressure;

$$\therefore \quad X^2\rho = 107600,$$

and K would be large if it were 100, so that even if a^2 were 50, still $2a^2K$ would still be less than $\frac{1}{10}^{\text{th}}$ of $X^2\rho$, so that we may take approximately

$$Q = kaKX;$$

$$\therefore \quad K = \frac{Q}{kaX}.$$

From this we can calculate K; for $k = 1{\cdot}6$ in most gases, and if $a = 2{\cdot}5$, and $X = 9700$ as above,

$$kaX = 38800 = \tfrac{1}{4} \times 10^4 \; q.p.$$

Now at a distance of a fourth metre in air at atmospheric pressure, and with a difference of temperature of 10° C.,

$Q = 10^6 \; q.p.$, so that in this case

$K = 25 \; q.p.$ which is within the limits of the quantities obtained in the case of the spheroidal drops of liquids.

That by this formula K varies nearly as Q, and not as Q^2, is not to be wondered at because in the first place the formula only professes to represent an approximation to the true state of affairs, and in the second place it is only at distances and pressures at which the ordinary laws of conduction of heat cease to apply that it professes even approximately to represent it.

The whole of these investigations are unsatisfactory to this extent, that I have been unable, from a consideration of the molecular encounters themselves, to discover what is the actual distribution of velocities even in the simple case of two parallel surfaces. This is hardly to be wondered at; for the problem is extremely complicated, and evidently depends upon the undecided point in molecular physics, namely, the proportion of the molecules encountering in a given direction that are thrown off in the various other directions. We might very well assume with Maxwell that they are uniformly distributed in every direction after the encounter, but even this does not simplify the question sufficiently to bring it within my present powers of solution.

ADDENDUM.

Note.—Since publishing this paper, Mr. Stoney has called my attention to a point in which I inconsiderately misunderstood his paper in my allusion to it, on the first paragraph of page 63. What he has shown in the paper referred to is not that the distribution of velocities represented by two streams of unpolarized gas is the ultimate state towards which the gas tends when the number of molecules is indefinitely diminished, but that as long as there are considerable numbers of molecules present the actual state of the gas lies between that represented by mutually non-interfering streams and an unpolarized state. As the number of molecules diminishes indefinitely, the tendency is towards a state which might be represented by the radii drawn from a common centre to two hemispheres of unequal radius turned in opposite directions, the one of larger radius being turned from the heater. In this case it is easy to see that there would be no difference between the pressures in the direction of the transference of heat and in the perpendicular direction, and consequently no Crookes's force.

6

ON THE SUPERFICIAL TENSION OF FLUIDS AND ITS POSSIBLE RELATION TO MUSCULAR CONTRACTIONS

[From the *Scientific Transactions* of the Royal Dublin Society, 1878. Read June 17, 1878. See Nos. 9 and 10 *infra*.]

I. SOME of the latest contributions to the subject of the superficial tension of fluids are due to M. Lippmann's very remarkable researches into the connexion between surface tension and the difference of electrical potential at the contact of dissimilar fluids.* In connexion with these the lately published experiments of Messrs. Ayrton and Perry (see Proc. Royal Society, vol. xxvii, p. 196) are of great interest, and their conclusions are especially gratifying to me, because years ago, in the spring of 1875, I made some experiments, which seemed to me to show that much of the observed electromotive force of contact was accompanied by chemical action. I remark in a note I took on Friday, 19th March of that year, on some experiments made with varnished zinc and copper condensers "..... there always seemed an electromotive force in the air near the Zn .... not like contact theory, for it resuscitated itself after bringing the poles to the same potential." Similarly Messrs. Ayrton and Perry consider that the electromotive force of contact of dissimilar substances is accompanied, in most cases, at least, by chemical action of some kind, although the amount is so small as to escape the ordinary means of analysis. Now, it seems to me that this sort of chemical attraction, which

* Annales de Chimie et Physique, 5me Serie, vol. xii, p. 265.

only culminates in chemical action in some cases, may be used to explain superficial tensions generally, and M. Lippmann's results. For instance, if we suppose the superficial tension to depend upon this chemical attraction, we can easily see how it is affected by the direction of the current passing from one surface to the other; for we know, by the phenomena of electrolysis, that the direction of the current alters, and even reverses, the character of this chemical attraction. Similarly Professor C. Maxwell mentions, in his article on Capillarity, in the last edition of the Encyclopædia Britannica, that when a mercury surface is being extended, there exists an accompanying electrical displacement. Sir W. Thomson, in his method of calculating the effective size of molecules by means of the observed difference of electrical potential on contact of zinc and copper, assumes that the actual chemical attractive forces are measurable by means of the attraction of the zinc and copper plates. From exactly similar premisses I propose calculating the superficial tension of a fluid. We require to know the electrical distribution corresponding to the contact of dissimilar substances, and I shall assume each molecule to be charged with that known quantity which passes when an electro-chemical equivalent is produced. Any such assumption as that the electrical distribution would produce the observed electrical potential is evidently inadmissible, because then the effects would vary with the forms of the surfaces, which is not the case. Hence, if χ be the quantity of electricity employed in producing one electro-chemical equivalent, and if n be the number of such equivalents in presence of one another per unit of area of surface in contact, and if A be the area of contact, and ϵ the electromotive force of contact, and T the superficial tension per unit of length, we have the two equivalent expressions for the superficial energy,

$$TA = n\chi A\epsilon \quad \text{or} \quad T = n\chi\epsilon.$$

To approximate to a numerical calculation we must make some assumption. Thus, if we take the case of water, and assume N to be the total number of electro-chemical equivalents

in a gramme, and if we employ the C. G. S. system of units,

$$T = 106 \frac{n}{N} \epsilon;$$

and if we assume T = ·08 gr. per centimetre, or = 78·5 dynes per centimetre, and $\epsilon = 10^7$ or the tenth of a volt, we have

$$\frac{\epsilon}{T} = \frac{10^7}{78{\cdot}5},$$

and consequently

$$\frac{n}{N} = \frac{78{\cdot}5}{106} 10^{-7} = 7{\cdot}3 \times 10^{-8},$$

so that, if this were a surface of unit area, and thickness θ so small that all the particles are within reach of the surface, we should have $n = N\theta$, and consequently $\theta = 7{\cdot}3 \times 10^{-8}$, which approximates towards the quantities obtained by other methods. The most doubtful assumption I have made is that the electromotive force of contact of air and water is a tenth of a volt, but this is not impossible.

I conclude, then, that superficial tension is solely due to this chemical attraction of dissimilar substances, as *some part of it, at least, must be due to this cause which produces a potential energy of the masses depending directly upon the area of the surfaces in contact.* I may mention that it seems to me likely that frictional electricity is due to a similar cause, and that it may be compared with Maxwell's experiment of the electrical current produced by altering the surface of mercury. In the same connexion compare Mr. Clark's experiments, as published in Wiedemann's Annalen der Physik und Chemie, Neue Folge, Band II, on the electrical potential of water driven through capillary tubes.*

II. I intend to devote this second part of my paper to developing the possible connexion between surface tension and muscular action.

[* For the subsequent development of this order of ideas, particularly of the notion of double electric layers, see Helmholtz, "Ueber Elektrische Grenzschichten," *Berlin. Monatsberichte*, Feb. 27, 1879, and *Wiedemann's Annalen* 7, 1879, pp. 337–382; also his Faraday Lecture, *Journal of the Chemical Society*, 1881.]

It is known that muscles when they contract, alter little or not at all in bulk, and it seems, therefore, likely that the alteration in arrangement that they undergo is a change of superficial area of their component parts. Now, if we assume a muscle to be composed of a number of circular fibres, each the $\frac{1}{100}$th of a centimetre in diameter, and each of these to consist of closely packed fibrillæ, and to facilitate computation, if we assume each fibrilla to be of a triangular section, and the side of each triangle to be the $\frac{1}{3000}$th of a centimetre in length, these quantities being about the amounts observed in Mammalian muscles, it is easy to calculate that there will be about 500 metres of circumference of fibrillæ per square centimetre of muscle which, with a superficial tension equal to that of water, gives a disposable force of four kilograms per square centimetre. The amount observed is about 7 kilograms per square centimetre,* and I think that what I have obtained comes sufficiently close to that observed for a more advantageous mode of distribution of the fibrillæ, or a slight diminution of their size, to account for the difference. Taking the thickness of the active superficial layer to be that obtained in the first part of this paper, the maximum force which could be obtained by making the structure as fine as possible, and the superficial tension that of water, would be nearly 1000 kilograms per square centimetre, so that there is plenty of margin for compensating diminished superficial tensions by increased fineness of structure. It is remarkable in this connexion that in frogs, whose muscles are more coarsely made, the maximum contractile force falls very much below that of the Mammalia. A system of elongated cylindrical fibrillæ would not be in stable equilibrium, but would break up into short lengths of less than three times their diameter, and this is just what is observed to be the case in all striated muscles. It has been questioned whether the fibrillar divisions and transverse striæ to be found in dead muscles have an actual existence in life. Yet I think there can be little doubt but that some structural peculiarity in life

* See Haughton's "Animal Mechanics," pp. 63–71.

corresponds to these sub-divisions, and any such would produce a superficies capable of developing superficial tension. It may seem improbable that as high a superficial tension as that of water can exist in this case, but the undoubted fact of considerable electrical disturbance accompanying muscular contraction, taken in connexion with M. Lippmann's experiments proving the connexion between differences of electrical potential and superficial tensions, very much diminishes the force of this objection. If we suppose a structure to consist of a series of ellipsoids of revolution of ellipticity = $\sin\theta$, and if r be the radius of the sphere whose volume is equal to that of each cell, then the force each would exert in the direction of its axis for surface tension = T is given by the equation

$$F = T\frac{\pi r \cos^4\theta}{2\sin^3\theta}\{\sin\theta\cos\theta(1+2\cos^2\theta) - \theta(4\cos^2\theta - 1)\},$$

and it is easy to see that there is some given form of ellipsoid of revolution of given volume for which this force will be a maximum, as it vanishes in the two extreme cases of a sphere and an infinite cylinder. If we calculate the pressure in a cylinder of radius r, whose sides are formed of a series of rings like anchor rings, we find that the pressure is given by the equation

$$P = \frac{\pi T}{2r},$$

and is independent of the section of each ring, so that, by piling a great number of very thin rings upon one another, a very great pressure might be developed, though of course the outer ones would be less effective on account of the greater value of r. The case of the heart is somewhat similar to this.

A constant supply of fresh blood is necessary to keep up the irritability of a muscle, because, after each contraction, the surfaces at which the superficial tension has increased have been altered chemically, and it is necessary to remove this debris and renew the surface in order to let the muscle relax. We see thus how it happens that a contracted muscle requires a constant stimulus to keep it so, and why keeping it contracted tires the muscle, though no external work is performed. For, as

there is, during life, a continual renewal of the surfaces of the fibrillæ, there is required a constant stimulus to keep them as continually changed into the altered state in which the superficial tension is increased, while after death *rigor mortis* sets in, because this altered state then becomes the permanent one.

A remarkable confirmation of my theory is that it completely explains the fact of a muscle heating when it contracts. Whenever the area of any fluid surface is increased it cools, and when it is diminished it heats, and this is true of all fluids yet observed, for the superficial tension uniformly diminishes when the temperature increases, and the law I have just mentioned is a direct consequence of this fact and of the laws of Thermodynamics.* Hence, when a muscle contracts, if this be accompanied by a diminution of superficial extension, we should expect it to heat, which is what actually takes place.

In the 58th number of the Quarterly Journal of Science, Dr. Stanley Jevons has published a most interesting paper upon what are known as the Brownian motions of small particles suspended in a fluid, and attributes them to a very slight chemical action producing electrical currents, but he does not explain how the currents produce motion. Now, although such a high authority as Faraday discarded surface tension as an explanation of these movements, nevertheless it has been reproposed by Tyndall as a possible explanation, and I think the motion of a drop of mercury in a horizontal glass tube when an electric current traverses some dilute acid surrounding the mercury is a very analogous phenomenon, only that the origin of the differences of electrical potential in the one case is external to the immersed substance, and, in the other case, is probably, as Dr. Jevons supposes, due to a very slight chemical action of the suspending fluid on the particle. This explanation is rendered the more probable by Messrs. Ayrton and Perry's having shown in the Proceedings of the Royal Society (*loc. cit.*) that almost every case of contact of dissimilar substances is

* See "The Theory of Heat," by Professor J. C. Maxwell, in the Text-Books of Science Series, p. 291.

accompanied by some chemical action. It is of course possible that these very small particles may be carried about by the continually moving ultimate molecules of the fluid, which are proved to be in constant motion by the phenomena of diffusion, but it is very improbable that such is the fact, though in that case no energy need be expended in order to keep up the motion which might consequently go on for ever. Considering the very complex chemical constitution of organic substances, it is similarly possible that muscular contractions are due to some rearrangement of the ultimate molecules constituting the muscle, but in that case it seems improbable that the volume would remain unaltered, and, besides, their very complicated structure would then seem unnecessary.

[Note added November 26, 1878.]

It may seem doubtful whether my assumption, that an electromotive force of contact exists at the surface of separation of a liquid and a gas, is justified, especially as Messrs. Ayrton and Perry seem to assert the contrary. Nevertheless, in a paper published in the Philosophical Magazine for August, 1878, Mr. Brown details some experiments which prove conclusively that there exists an electromotive force of contact at the surface of separation of a metal and a gas, and probably the same is true of a liquid and a gas. In the case he mentions, the direction of the electromotive force between copper and iron was reversed by substituting sulphide of hydrogen for air, and this shows that these electromotive forces are at least comparable to those which have been hitherto assumed to be due to the contact of the metals with one another.

It seems probable that the cohesion of molecules in matter and of groups of atoms in binary compounds are phenomena of the same kind.

Considering the enormous liquid surface produced in the form of spray by Armstrong's electrical machine, it seems reasonable to explain its efficiency by the electrical displacements accompanying the enlargement and diminution of liquid surfaces.

7

ON THE ELECTROMAGNETIC THEORY OF THE REFLECTION AND REFRACTION OF LIGHT

[From the *Proceedings of the Royal Society*, 1879. Received October 26, 1878. Abstract of No. 8 *infra*.]

THE media, at whose surfaces reflection and refraction are supposed to take place, are assumed to be non-conductors, and isotropic as regards magnetic inductive capacity. Some reasons are advanced why the results should apply at least approximately to conductors. In the first part of the paper the media are not assumed to be isotropic as regards electrostatic inductive capacity, so that the results are generally applicable to reflection and refraction at the surfaces of crystals. I use the expressions given by Professor J. Clerk Maxwell in his "Electricity and Magnetism," vol. ii, part 4, chap 11, for the electrostatic and electrokinetic energy of such media. By assuming three quantities, ξ, η, ζ, such that, t representing time, $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, and $\frac{d\zeta}{dt}$ are the components of the magnetic force at any point, I have thrown these expressions for the electrostatic and electrokinetic energy of a medium into the same forms as M'Cullagh assumed to represent the potential and kinetic energy of the ether, in "An Essay towards a Dynamical Theory of Crystalline Reflection and Refraction," published in vol. xxi of the "Transactions of the Royal Irish Academy." Following a slightly different line from his, I obtain, by a quaternion and accompanying Cartesian analysis, the same results as to wave propagation, reflection, and refraction, as those obtained by M'Cullagh, and which he developed

into the beautiful theorem of the polar plane. Of course, the resulting laws of wave propagation agree with those obtained by Professor Maxwell from the same equations by a somewhat different method. For isotropic media, the ordinary laws of reflection and refraction are obtained, and the well-known expressions for the amplitudes of the reflected and refracted rays.

In the second part of the paper I consider the case of reflection at the surface of a magnetised medium, adopting the expressions Professor J. Clerk Maxwell has assumed in "Electricity and Magnetism," vol. ii, part 4, § 824, to express the kinetic energy of such a medium. From this, following the same line as before, I have deduced the following equations to represent the superficial conditions. In them, ξ, η, ζ have the same meaning as before, and the axes are x in the intersection of the plane of incidence and the surface, y in the surface, and z normal to it; α, β, γ are the components of the strength of the vortex that Professor Maxwell assumes, and

$$\frac{d}{dh} = \alpha \frac{d}{dx} + \beta \frac{d}{d\gamma} + \gamma \frac{d}{dz},$$

which, with these axes, reduces to

$$\alpha \frac{d}{dx} + \gamma \frac{d}{dz};$$

K and K_1 are the electrostatic inductive capacities of the two media in contact, and the quantities referring to one of these which is supposed to be non-magnetic are distinguished by the suffix $_1$; C is a constant, on which the power of the medium to rotate the plane of polarization of light depends.

$$\frac{d\xi_1}{dz_1} - \frac{d\zeta_1}{dx_1} = \frac{K_1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) - 4\pi C K_1 \left\{\gamma \frac{d^2\eta}{dzdt} + \frac{d^2\eta}{dhdt}\right\}$$

$$\frac{d\eta_1}{dz_1} = \frac{K_1}{K}\frac{d\eta}{dz} + 4\pi C K_1 \left\{\gamma \frac{d}{dt}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + \frac{d^2\xi}{dhdt}\right\}.$$

As these are unchanged by a simultaneous alteration of the signs of η and C, I show that the method adopted in my former

paper on "Magnetic Reflection," in the Proceedings of the Royal Society for 1876, No. 176, is justified, and that it is legitimate to consider an incident plane polarized ray as composed of two oppositely circularly polarized rays, each of which is reflected according to its own laws. From these I further deduce that, when the magnetization of the medium is all in the direction of η, there is no effect on reflection or refraction produced by it. I consider next the cases of the magnetization being all normal to the surface, and all in the surface and the plane of incidence, and obtain the following result:—When the incident ray is plane polarized, and the plane of polarization is either in or perpendicular to the plane of incidence, the effect of magnetization is to introduce a component into the reflected ray perpendicular to the original plane of polarization, whose amplitude, c, is given in the several cases by the following equations, in which i is the angle of incidence, and r of reflection, and k a small constant depending principally on C and the intensity of the incident ray:—1. When the magnetization is normal to the reflecting surface.* If the incident ray be polarized in the plane of incidence

$$c = k\frac{(1+\cos^2 r)\sin^2 i \sin 2i}{\sin r \sin^2(i+r)\cos(i-r)}.$$

If it be polarized in a plane perpendicular to the plane of incidence

$$c = 2k\frac{\cos^2 r \sin^2 i \sin 2i}{\sin r \sin^2(i+r)\cos(i-r)}$$

2. When the magnetization is parallel to the intersection of the surface and the plane of incidence, and the plane of polarization of the incident ray is either in or perpendicular to the plane of incidence

$$c = k\frac{\cos r \sin^2 i \sin 2i}{\sin^2(i+r)\cos(i-r)}$$

This vanishes at the grazing and normal incidences, and, in the

[* For corrections see footnote, p. 63.]

case of iron, attains a maximum at about the angle of incidence $i = 63° 20'$.

I do not obtain any change of phase by reflection in any case; and this is to be expected, as this change of phase probably depends on the nature of the change from one medium to another, which, following M'Cullagh, I have uniformly assumed to be abrupt.* Apart from this question of change of phase, my results conform completely to Mr. Kerr's beautiful experiments on the reflection of light from the pole of a magnet, as published in the Philosophical Magazine for May, 1877, and March, 1878.

[* See footnote, p. 66.]

8

ON THE ELECTROMAGNETIC THEORY OF THE REFLECTION AND REFRACTION OF LIGHT

[From the *Philosophical Transactions of the Royal Society* (Pt. II, 1880; Art. XIX, p. 691). Communicated by G. J. Stoney, M.A., F.R.S., Secretary of the Queen's University in Ireland. Received October 26, 1878. Read January 9, 1879.]

IN the second volume of his "Electricity and Magnetism," Professor J. Clerk Maxwell has proposed a very remarkable electromagnetic theory of light, and has worked out the results as far as the transmission of light through uniform crystalline and magnetic media are concerned, leaving the questions of reflection and refraction untouched. These, however, may be very conveniently studied from his point of view.

If we call W the electrostatic energy of the medium, it may be expressed in terms of the electromotive force and the electric displacement at each point, as is done in Professor Maxwell's "Electricity and Magnetism," vol. ii, part 4, ch. 9. I shall adopt his notation, and call the electromotive force $\mathfrak{E}$, and its components P, Q, R; and the electric displacement $\mathfrak{D}$, and its components f, g, h. As several of the results of this Paper admit of a very elegant expression in Quaternion notation, I shall give the work and results in both Cartesian and Quaternion form, confining the German letters to the Quaternion notation. Between these quantities, then, we have the equation

$$W = -\tfrac{1}{2}\iiint S\mathfrak{E}\mathfrak{D}\,dxdydz = \tfrac{1}{2}\iiint (Pf + Qg + Rh)\,dxdydz.$$

Similarly, the kinetic energy T may be expressed in terms

of the magnetic induction $\mathfrak{B}$, and the magnetic force $\mathfrak{H}$, or their components a, b, c, and α, β, γ, by the equation

$$T = -\frac{1}{8\pi}\iiint S\mathfrak{B}\mathfrak{H}\,dxdydz = \frac{1}{8\pi}\iiint (a\alpha + b\beta + c\gamma)\,dxdydz.$$

I shall at present assume this to be a complete expression for T, and return to the case of magnetized media for separate treatment, as Professor Maxwell has proposed additional terms in this case, in order to account for their property of rotatory polarization. I shall throughout assume the media to be isotropic as regards magnetic induction, for the contrary supposition would enormously complicate the question, and be, besides, of doubtful physical applicability. For the present I shall not assume them to be electrostatically isotropic. Hence $\mathfrak{E}$ is a linear vector and self-conjugate function of $\mathfrak{D}$, and consequently P, Q, R linear functions of f, g, h, so that we may write in Quaternion notation

$$\mathfrak{E} = \phi\mathfrak{D};$$

and if we call U the general symmetrical quadratic function of f, g, h, we may assume

$$U = Pf + Qg + Rh,$$

and consequently

$$W = -\tfrac{1}{2}\iiint S\mathfrak{D}\phi\mathfrak{D}\,dxdydz = \tfrac{1}{2}\iiint U\,dxdydz.$$

As the medium is magnetically isotropic, we have

$$\mathfrak{B} = \mu\mathfrak{H}, \quad \text{or} \quad a = \mu\alpha, \quad b = \mu\beta, \quad c = \mu\gamma,$$

where μ is the coefficient of magnetic inductive capacity, and consequently the electrokinetic energy may be written

$$T = -\frac{\mu}{8\pi}\iiint \mathfrak{H}^2\,dxdydz = \frac{\mu}{8\pi}\iiint (\alpha^2 + \beta^2 + \gamma^2)\,dxdydz.$$

Now I shall assume the mediums to be non-conductors; and although this limits to some extent the applicability of my

results, and notably their relation to metallic reflection, yet it is a necessity, for otherwise the problem would be beyond my present powers of solution. With this assumption, and using Newton's notation of $\dot{x}$ for $\frac{dx}{dt}$, we have the following equations (see "Electricity and Magnetism," vol. ii., § 619):—

$$4\pi\dot{\mathfrak{D}} = V\nabla\mathfrak{H},$$

using ∇ for the operation

$$i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz};$$

or the same in terms of its components, namely,

$$4\pi\dot{f} = \frac{d\gamma}{dy} - \frac{d\beta}{dz},$$

$$4\pi g = \frac{d\alpha}{dz} - \frac{d\gamma}{dx},$$

$$4\pi\dot{h} = \frac{d\beta}{dx} - \frac{d\alpha}{dy}.$$

Assuming now a quantity $\mathfrak{R}$ with components ξ, η, ζ, such that

$$\mathfrak{R} = \int\mathfrak{H}dt,$$

and consequently

$$\dot{\mathfrak{R}} = \mathfrak{H};$$

or, in terms of the components,

$$\dot{\xi} = \alpha, \quad \dot{\eta} = \beta, \quad \dot{\zeta} = \gamma,$$

we may evidently write

$$4\pi\mathfrak{D} = V\nabla\mathfrak{R},$$

i.e., $$4\pi f = \frac{d\zeta}{dy} - \frac{d\eta}{dz}, \quad 4\pi g = \frac{d\xi}{dz} - \frac{d\zeta}{dx}, \quad 4\pi h = \frac{d\eta}{dx} - \frac{d\xi}{dy},$$

so that we have

$$W = -\frac{1}{32\pi^2}\iiint S(V\nabla\mathfrak{R}\,.\,\phi V\nabla\mathfrak{R})\,dxdydz,$$

$$T = -\frac{\mu}{8\pi}\iiint\dot{\mathfrak{R}}^2\,dxdydz = \frac{\mu}{8\pi}\iiint(\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2)\,dxdydz.$$

Lagrange's equations of motion may often be very conveniently represented as the conditions that $\int(T - W)\,dt$ should be a minimum, or, in other words, that

$$\delta \int (T - W)\,dt = 0,$$

and this method, from its symmetry, is particularly applicable to the methods of Quaternions.

Proceeding on this method we obtain immediately the equation

$$0 = -\int\left[\mu\iiint S\ddot{\mathfrak{R}}\delta\mathfrak{R}\,dxdydz - \frac{1}{4\pi}\iiint S(V\nabla\delta\mathfrak{R}\,\phi\,V\nabla\mathfrak{R})\,dxdydz\right]dt,$$

or in Cartesian notation,

$$0 = \int\left[\frac{\mu}{4\pi}\iiint(\ddot{\xi}\delta\xi + \ddot{\eta}\delta\eta + \ddot{\zeta}\delta\zeta)\,dxdydz - \tfrac{1}{3}\iiint\left(\frac{dU}{df}\delta f + \frac{dU}{dg}\delta g + \frac{dU}{dh}\delta h\right)dxdydz\right]dt.$$

Now we may evidently integrate the terms in $\delta\ddot{\mathfrak{R}}$ and $\delta\ddot{\xi}$, $\delta\ddot{\eta}$, $\delta\ddot{\zeta}$ with reference to the time, and the terms depending on the limits of the time must vanish separately, and we are not at present concerned with them, so that the equation reduces to

$$\iiiint\left[\mu S\ddot{\mathfrak{R}}\delta\mathfrak{R} + \frac{1}{4\pi}S(V\nabla\delta\mathfrak{R}\,\phi\,V\nabla\mathfrak{R})\right]dxdydzdt = 0,$$

or

$$\iiiint\left[\frac{\mu}{4\pi}(\ddot{\xi}\delta\xi + \ddot{\eta}\delta\eta + \ddot{\zeta}\delta\zeta) + \tfrac{1}{3}\left(\frac{dU}{df}\delta f + \frac{dU}{dg}\delta g + \frac{dU}{dh}\delta h\right)\right]dxdydzdt = 0.$$

I shall now proceed to integrate this by parts relatively to x, y, z, and in order to express the result conveniently I shall assume ds to be an element of the surface of the medium and $\mathfrak{R}$, with components l, m, n, to be a unit normal to this element of surface, when we evidently obtain

$$\int\left[\frac{1}{4\pi}\iint S(\mathfrak{R}\,\phi\,V\nabla\mathfrak{R}\delta\mathfrak{R})\,ds + \iiint\left\{\mu S\ddot{\mathfrak{R}}\delta\mathfrak{R} + \frac{1}{4\pi}S(V\nabla\phi\,V\nabla\ \delta\mathfrak{R})\right\}dxdydz\right]dt = 0,$$

or

$$\int \left\{ \begin{aligned} &\mu \iiint (\ddot{\xi}\delta\xi + \ddot{\eta}\delta\eta + \ddot{\zeta}\delta\zeta)\, dxdydz \\ &+ \tfrac{1}{8\pi} \iiint \left[\left(\frac{d}{dy}\frac{dU}{dh} - \frac{d}{dz}\frac{dU}{dg} \right) \delta\xi + \left(\frac{d}{dz}\frac{dU}{df} - \frac{d}{dx}\frac{dU}{dh} \right) d\eta \right. \\ &\qquad \left. + \left(\frac{d}{dx}\frac{dU}{dg} - \frac{d}{dy}\frac{dU}{df} \right) \delta\zeta \right] dxdydz \\ &+ \tfrac{1}{8\pi} \iint \left[\left(n\frac{dU}{dg} - m\frac{dU}{dh} \right) \delta\xi + \left(l\frac{dU}{dh} - n\frac{dU}{df} \right) d\eta \right. \\ &\qquad \left. + \left(m\frac{dU}{df} - l\frac{dU}{dg} \right) \delta\zeta \right] ds \end{aligned} \right\} dt = 0.$$

It is evident that the superficial and general integrals must vanish separately, and as $\delta\mathfrak{R}$ or $\delta\xi$, $\delta\eta$, and $\delta\zeta$ is arbitrary in the general integrals, we must evidently have

$$4\pi\mu\ddot{\mathfrak{R}} + V\nabla\phi\, V\nabla\mathfrak{R} = 0,$$

or

$$\mu\ddot{\xi} + \tfrac{1}{8\pi}\left(\frac{d}{dy}\frac{dU}{dh} - \frac{d}{dz}\frac{dU}{dg} \right) = 0,$$

$$\mu\ddot{\eta} + \tfrac{1}{8\pi}\left(\frac{d}{dz}\frac{dU}{df} - \frac{d}{dx}\frac{dU}{dh} \right) = 0,$$

$$\mu\ddot{\zeta} + \tfrac{1}{8\pi}\left(\frac{d}{dx}\frac{dU}{dg} - \frac{d}{dy}\frac{dU}{df} \right) = 0.$$

As it will not take long I will deduce the ordinary equations for the transmission of plane-waves from these before proceeding to discuss the superficial condition. In the first place, as our axes of coordinates are perfectly arbitrary, I shall assume z to be normal to the plane of the wave, and consequently ξ, η, ζ to be functions of z and t only, so that $\nabla = k\frac{d}{dz}$ and $\frac{d}{dx} = \frac{d}{dy} = 0$, and consequently $\ddot{\zeta} = 0$, and $h = 0$, and

$$V\nabla\mathfrak{R} = i\frac{d\eta}{dz} - j\frac{d\xi}{dz};$$

so that if $$\phi\rho = \lambda S i\rho + \mu S j\rho + \nu S k\rho,$$

$$\phi V\nabla\mathfrak{R} = \mu\frac{d\xi}{dz} - \lambda\frac{d\eta}{dz},$$

and consequently

$$4\pi\mu\ddot{\mathfrak{R}} = Vk\mu\frac{d^2\xi}{dz^2} - Vk\lambda\frac{d^2\eta}{dz^2}.$$

Now, if we had originally assumed

$$U = Af^2 + Bg^2 + Ch^2 + 2Fgh + 2Ghf + 2Hfg,$$

we should have

$$\lambda = Ai + Hj + Gk, \quad \mu = Hi + Bj + Fk,$$

so that we obtain

$$4\pi\mu(i\ddot{\xi} + j\ddot{\eta} + k\ddot{\zeta}) + i\left(B\frac{d^2\xi}{dz^2} - H\frac{d^2\eta}{dz^2}\right) + j\left(A\frac{d^2\eta}{dz^2} - H\frac{d^2\xi}{dz^2}\right) = 0;$$

so that, if we assume i and j parallel to the axes of the section of $S\rho\phi\rho = 1$ by the wave-plane we evidently have got $H = 0$, and consequently $\ddot{\zeta} = 0$, and

$$4\pi\mu\ddot{\xi} = B\frac{d^2\xi}{dz^2}, \quad 4\pi\mu\eta = A\frac{d^2\eta}{dz^2};$$

and as A and B are inversely proportional to the squares of the axes of the section of $S\rho\phi\rho = 1$, or $U = 1$, by the wave-plane, we get M'Cullagh's result, that each component in these directions is propagated independently, and with a velocity inversely proportional to the perpendicular axis of the section of $S\rho\phi\rho = 1$ by the wave-plane.

This comes out at once from the Cartesian equations; for, as $h = 0$, U reduces to

$$U = Af^2 + 2Hfg + Bg^2;$$

and by choosing x and y parallel to the axes of this section, we have $H = 0$, so that

$$\frac{dU}{df} = 2Af, \quad \frac{dU}{dg} = 2Bg,$$

and

$$4\pi f = -\frac{d\eta}{dz}, \quad \text{and} \quad 4\pi g = \frac{d\xi}{dz},$$

and the result of putting these in is evidently the same as before.

Returning now to the superficial conditions, I shall follow M'Cullagh, and assume that at each point of the surface of separation of two media the values of the elements of the integrals must be equal for the two media, and, indeed, I think it is pretty evident that, if the original integrals are to express the whole state of affairs in the case of a motion propagated from one medium into another, the superficial integrals must vanish when the limits introduced in them are the functions corresponding to the two contiguous media.

Using the suffix $_0$ for one medium, and $_1$ for the other, and assuming the normal to the surface as k, we get, as $\delta\mathfrak{R}$ is evidently arbitrary, and the same for both media at the surface of separation,

$$\mathrm{V}k\phi_0\mathrm{V}\nabla\mathfrak{R} = \mathrm{V}k\phi_1\mathrm{V}\nabla\mathfrak{R},$$

or the two equations

$$\mathrm{S}i\phi_0\mathrm{V}\nabla\mathfrak{R} = \mathrm{S}i\phi_1\mathrm{V}\nabla\mathfrak{R},$$

$$\mathrm{S}j\phi_0\mathrm{V}\nabla\mathfrak{R} = \mathrm{S}j\phi_0\mathrm{V}\nabla\mathfrak{R},$$

which are the same as can be got at once by putting $n = 1$, $m = l = 0$ in the Cartesian superficial equations, and taking $\delta\xi$ and $\delta\eta$ as arbitrary and independent, when we get

$$\frac{d\mathrm{U}}{df_0} = \frac{d\mathrm{U}}{df_1}, \quad \frac{d\mathrm{U}}{dg_0} = \frac{d\mathrm{U}}{dg_1}.$$

If now we assume that the axis of x is the line of intersection of the plane of incidence and the surface, we may evidently assume $\mathfrak{R}_0$ to be the resultant of an incident and reflected ray, and $\mathfrak{R}_1$ to be the resultant of the two refracted rays, so that we may write

$$\mathfrak{R}_0 = j_0\eta_0 + j'_0\eta'_0, \quad \mathfrak{R}_1 = j_1\eta_1 + j'_1\eta'_1,$$

when j_0, j'_0, j_1, j'_1 are, respectively, unit-vectors parallel to the direction of magnetic displacement in the incident, reflected, and each refracted ray. Similarly, calling k_0, k'_0, k_1, k'_1 the unit-

vectors normal to these wave-planes, and z_0, z'_0, z_1, z'_1 the variable distance along these, evidently we may assume

$$\nabla_0 = k_0 \frac{d}{dz_0}, \quad \nabla'_0 = k'_0 \frac{d}{dz'_0}, \text{ \&c.};$$

and substituting these we at once obtain

$$\frac{d\eta_0}{dz_0} \mathrm{S}i\phi_0 i_0 + \frac{d\eta'_0}{dz'_0} \mathrm{S}i\phi_0 i'_0 = \frac{d\eta_1}{dz_1} \mathrm{S}i\phi_1 i_1 + \frac{d\eta'_1}{dz'_1} \mathrm{S}i\phi_1 i_1,$$

$$\frac{d\eta_0}{dz_0} \mathrm{S}j\phi_0 i_0 + \frac{d\eta'_0}{dz'_0} \mathrm{S}j\phi_0 i'_0 = \frac{d\eta_1}{dz_1} \mathrm{S}j\phi_1 i_1 + \frac{d\eta'_1}{dz'_1} \mathrm{S}j\phi_1 i'_1.$$

In these I shall now assume $\phi_0 = 1$, as it is convenient to suppose this medium to be isotropic and the velocity of propagation in it unity, and i_0 and i'_0 are the directions in the wave-plane perpendicular to the magnetic displacement; so that if $\alpha_0, \beta_0, \gamma_0, \alpha'_0, \beta'_0, \gamma'_0$ be the direction angles of these lines referred to the superficial axes

$$\mathrm{S}i\phi_0 i_0 = \cos\alpha_0, \quad \mathrm{S}i\phi_0 i'_0 = \cos\alpha'_0,$$

$$\mathrm{S}j\phi_0 i_0 = \cos\beta_0, \quad \mathrm{S}j\phi_0 i'_0 = \cos\beta'_0;$$

and if s^{-1} be that axis of the section of $\mathrm{S}\rho\phi\rho = 1$, by one of the refracted wave-planes which is perpendicular to the direction of magnetic displacement, it is evidently the velocity of propagation of this wave in this medium, and we may write

$$i_1 = \mathrm{U}s = s^{-1}\mathrm{T}s; \quad \text{therefore} \quad \phi i_1 = \phi s^{-1}\mathrm{T}s = v\mathrm{T}s,$$

when v^{-1} is the perpendicular on the corresponding tangent plane, and consequently

$$\mathrm{S}i\phi i_1 = \mathrm{T}v\mathrm{T}s \cos\alpha_1,$$

when α_1 is the angle between v and the axis of x, and evidently $\mathrm{T}s$ represents the velocity of this wave, while $\mathrm{V}vj$ is the direction of the ray. Hence our equations may be written

$$\cos\alpha_0 \frac{d\eta_0}{dz_0} + \cos\alpha'_0 \frac{d\eta'_0}{dz'_0} = \mathrm{T}v\mathrm{T}s \cos\alpha_1 \frac{d\eta_1}{dz_1} + \mathrm{T}v'\mathrm{T}s' \cos\alpha'_1 \frac{d\eta'_1}{dz'_1},$$

$$\cos\beta_0 \frac{d\eta_0}{dz_0} + \cos\beta'_0 \frac{d\eta'_0}{dz'_0} = \mathrm{T}v\mathrm{T}s \cos\beta_1 \frac{d\eta_1}{dz_1} + \mathrm{T}v'\mathrm{T}s' \cos\beta'_1 \frac{d\eta'_1}{dz'_1}.$$

These may be readily deduced from the Cartesian equations as follows: U_0 being isotropic it can be written

$$= A_0(f_0^2 + g_0^2 + h_0^2),$$

and is evidently unaltered by transformation; and if $l : m : n$ and $l_1 : m_1 : n_1$ be the direction cosines of x and y to any arbitrary axes, we get

$$A_0 f_0 = l\frac{dU_1}{df} + m\frac{dU_1}{dg} + n\frac{dU_1}{dh},$$

$$A_0 g_0 = l_1\frac{dU_1}{df} + m_1\frac{dU_1}{dg} + n_1\frac{dU_1}{dh};$$

and now, as these are linear, we may suppose the superficial disturbance in one medium to be due to an incident and reflected wave, and in the other to two refracted rays, so that we can write these as

$$A_0(f_0 + f'_0) = \left(l\frac{dU_1}{df} + m\frac{dU_1}{dg} + n\frac{dU_1}{dh}\right) + \left(l'\frac{dU'_1}{df} + m'\frac{dU'_1}{dg} + n'\frac{dU'_1}{dh}\right),$$

$$A_0(g_0 + g'_0) = \left(l_1\frac{dU_1}{df} + m_1\frac{dU_1}{dg} + n_1\frac{dU}{dh_1}\right) + \left(l'_1\frac{dU'_1}{df_1} + m'_1\frac{dU'_1}{dg_1} + n'_1\frac{dU'_1}{dh}\right);$$

and as U and U'_1 are supposed to be referred to arbitrary axes, we may suppose U_1 to be referred to such that η_1 is the only component—*i. e.*, to such that the direction of the magnetic force is the axis of y, that of z being normal to the wave-plane, and similarly η'_1 being the only component in U'_1, while the z axis in this case is normal to its wave-plane, and of course this η_1 will be a function of z_1 only, and η'_1 of z'_1 only, these being the corresponding ordinates. We thus obtain

$$4\pi f_1 = -\frac{d\eta_1}{dz_1}, \quad 4\pi f'_1 = -\frac{d\eta'_1}{dz'_1},$$

$$g_1 = g'_1 = h_1 = h'_1 = 0,$$

so that

$$\frac{dU_1}{df_1} = A_1 f_1, \quad \frac{dU'_1}{df'_1} = A'_1 f'_1, \quad \frac{dU_1}{dg_1} = H_1 f_1,$$

$$\frac{dU'_1}{dg'_1} = H'_1 f'_1, \quad \frac{dU_1}{dh_1} = G_1 f_1, \quad \frac{dU'_1}{dh'_1} = G'_1 f'_1;$$

and the equations become

$$-4\pi A_0(f_0+f'_0) = (lA_1+mH_1+nG_1)\frac{d\eta_1}{dz_1} + (l'A'_1+m'H'_1+n'G'_1)\frac{d\eta'_1}{dz'_1},$$

$$-4\pi A_0(g_0+g'_0) = (l_1A_1+m_1H_1+n_1G_1)\frac{d\eta_1}{dz_1} + (l'_1A'_1+m'_1H'_1+n'_1G'_1)\frac{d\eta'_1}{dz'_1}.$$

Now if we consider A_1, H_1, G_1 we see that they are proportional to the direction cosines of the perpendicular on the tangent plane to U where it is met by $g = 0$, $h = 0$—*i.e.*, it is the direction conjugate to the electric displacement corresponding to η_1. Now if we call $A_0 s$ the velocity of propagation of this wave and $A_0 s'$ of the other, we see at once that s^{-1} is an axis of the section of U = a constant by one wave-plane, and s'^{-1} is an axis of the section by the other wave-plane, and if v^{-1} and v'^{-1} be the corresponding conjugate directions, and α_1 and α'_1, β_1 and β'_1 the angles between these latter and the superficial x and y axes, and if α_0, β_0, α'_0, β'_0 be the corresponding angles between η_0 and η'_0 and these same superficial axes, we get exactly the same equations as before, and write

$$\frac{d\eta_0}{dz_0}\cos\alpha_0 + \frac{d\eta'_0}{dz'_0}\cos\alpha'_0 = vs\cos\alpha_1\frac{d\eta_1}{dz_1} + v's'\cos\alpha'_1\frac{d\eta'_1}{dz'_1}.$$

$$\frac{d\eta_0}{dz_0}\cos\beta_0 + \frac{d\eta'_0}{dz'_0}\cos\beta'_0 = vs\cos\beta_1\frac{d\eta_1}{dz_1} + v's'\cos\alpha'_1\frac{d\eta'_1}{dz'_1}$$

In order to reduce these further we assume

$$\eta_0 = T_0\cos\frac{2\pi}{\lambda_0}(t - z_0), \qquad \eta'_0 = T'_0\cos\frac{2\pi}{\lambda'_0}(t - z'_0),$$

$$\eta_1 = T_1\cos\frac{2\pi}{\lambda_1}(st - z_1), \qquad \eta'_1 = T'_1\cos\frac{2\pi}{\lambda'_1}(s't - z'_1);$$

and if our superficial axes are so placed that the axis of x is the intersection of the plane of incidence and the surface, and if

i_0, i'_0, i_1, i'_1 be the angles the respective wave normals make with the normal to the surface, we evidently may write

$$z_0 = z\cos i_0 + x\sin i_0, \qquad z'_0 = z\cos i'_0 + x\sin i'_0,$$
$$z_1 = z\cos i_1 + x\sin i_1, \qquad z'_1 = z\cos i'_1 + x\sin i'_1,$$

as it is easy to convince oneself that any terms involving y would be inadmissible, as they could not by hypothesis occur in z_0, and the terms involving the time could not vanish out of our equations if they occurred in the others. Hence these wave normals are all in the same plane. When $z = 0$ our equations must evidently be true independently of the time and x, from which we see that no change of phase is possible in reflection. Hence these equations cannot explain metallic reflection. Indeed, this question of change of phase seems to be one of a higher order than I am here dealing with, and requires a discussion of the nature of the transition from one medium to another, which, of course, cannot be abrupt, as our equations suppose, nor indeed probably, in these cases, even very small compared with the vibrations.* In order that these terms involving the time should disappear from the equations when $z = 0$, we must have

$$\lambda_0 : \lambda'_0 : \lambda_1 : \lambda'_1 :: \sin i_0 : \sin i'_0 : \sin i_1 : \sin i'_1 :: 1 : 1 : s : s',$$

which involves that the angle of incidence should be equal to the angle of reflection; and if the second medium were an ordinary one, so that $s = \frac{1}{\rho} = s'$, we should have that the ratio of the sines of the angles of incidence and refraction was constant. Putting in these values, our equations reduce to

$$\mathrm{T}_0\cos a_0 + \mathrm{T}'_0\cos a'_0 = v\mathrm{T}_1\cos a_1 + v'\mathrm{T}'_1\cos a'_1,$$
$$\mathrm{T}_0\cos\beta_0 + \mathrm{T}'_0\cos\beta'_0 = v\mathrm{T}_1\cos\beta_1 + v'\mathrm{T}'_1\cos\beta'_1,$$

together with the condition that when $z = 0$ the superficial displacements should be the same to whichever medium they belong, namely, $\xi = \xi'$, $\eta = \eta'$, $\zeta = \zeta'$.

[* The explanation, obtained as usual by simply making the refractive index a complex quantity, involves disturbance penetrating into the metallic medium to a distance comparable with the wave-length, which is doubtless the "transition" here referred to. See p. 70.]

As each of these is a resolved part of the vibrations η_0, η'_0, and η_1, η'_1 we get three additional equations, the last of which, however, is the same as the second of the former ones, and there result, consequently, but four equations from which the four quantities, namely the three intensities T'_0, T_1, T'_1, and the azimuth of T'_0, are to be determined.

It is remarkable that whether we assumed or no that $\zeta = \zeta'$ it is here introduced. That is, however, no proof that it is wrong to omit it, as in Fresnel's method of obtaining the intensities of the reflected and refracted rays, for the fact of its turning up independently shows that there is something at least debatable about it; and as I shall have cause to omit this equation as leading to inconvenient results in a subsequent part of my paper, I thought it well to mention that there is something curious about it even here. These equations are those long ago given by M'Cullagh in the Transactions of the Royal Irish Academy, vol. xxi, to solve the problem of crystalline reflection and refraction, and from which he deduces his beautiful theorem of the polar plane and thus marvellously simplifies an extremely complicated problem. Indeed, as the forms into which I have thrown T and W are identical with his expressions for what are practically the same quantities, my whole investigation so far is but a modification of his.

In the simple case of the second medium being also isotropic we have that $v = v' = s = s'$; and if we in the first place suppose η_0 to be in the plane of incidence, we have at once $\alpha_0 = 90°$, and consequently $\alpha'_0 = \alpha' = 90°$, and T'_1 vanishes, or at least may be supposed to do so, the medium being isotropic and $s = s'$, so that the two refracted waves coincide. Also $\beta_0 = 0$ and $\beta'_0 = \beta_1 = 0$ likewise, as can easily be seen by assuming that the reflected and refracted waves have components out of the plane of incidence, and then trying to satisfy the equations. Hence our equations reduce to

$$T_0 + T'_0 = \frac{1}{\rho^*} T_1,$$

and $\xi_0 = \xi'_0$ becomes $(T_0 - T'_0) \cos i = T_1 \cos r$,

* Where ρ is the refractive index.

i being the angle of incidence and r of reflection. The first of these is

$$(T_0 + T'_0) \sin i = T_1 \sin r;$$

and, solving them, we get

$$T'_0 = -T_0 \frac{\sin(i-r)}{\sin(i+r)}, \quad T_1 = T_0 \frac{\sin 2i}{\sin(i+r)}.$$

If η_0 be perpendicular to the plane of incidence we obtain $\alpha_0 = i$, $\beta_0 = 0$, and our equations become

$$T_0 + T'_0 = T_1$$

and

$$(T_0 - T'_0) \sin i \cos i = T_1 \sin r \cos r,$$

which give

$$T'_0 = T_0 \frac{\tan(i-r)}{\tan(i+r)}, \quad T_1 = T_0 \frac{\sin 2i}{\sin(i+r)\cos(i-r)}.$$

Having now deduced the already known laws of reflection and refraction of light at crystalline and ordinary surfaces, I shall proceed to consider the case that Mr. Kerr's wonderfully beautiful experiments have made so interesting—namely, the case of reflection from magnetic surfaces. In order to do this I shall assume, with Professor J. Clerk Maxwell (see "Electricity and Magnetism," vol. ii, § 824), that the kinetic energy of the medium contains a term depending on the displacement of certain supposed vortices, and that it may be expressed by an equation of the form (§ 826)

$$T = -C \iiint S\left(\frac{d\mathfrak{R}}{d\theta} V \nabla \dot{\mathfrak{R}}\right) dxdydz$$

$$= 4\pi C \iiint \left(\frac{d\xi}{d\theta}\dot{f} + \frac{d\eta}{d\theta}\dot{g} + \frac{d\zeta}{d\theta}\dot{h}\right) dxdydz$$

where

$$\frac{d}{d\theta} = \alpha \frac{d}{dx} + \beta \frac{d}{dy} + \gamma \frac{d}{dz},$$

and α, β, γ are now the components of the vortex $\mathfrak{M}$.

I shall assume the medium to be isotropic, so that taking $\phi = \frac{4\pi}{K}$ the electrostatic energy of the medium may be expressed as

$$W = -\frac{1}{8\pi K} \iiint (V \nabla \mathfrak{R})^2 dxdydz$$

$$= \frac{1}{8\pi K} \iiint \left[\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right)^2 + \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right)^2 + \left(\frac{d\eta}{dx} - \frac{d\xi}{dy}\right)^2\right] dxdydz;$$

while the complete value of T is

$$T = -\frac{1}{8\pi}\iiint\left[\mu\dot{\mathfrak{R}}^2 + 8\pi CS\left(\frac{d\mathfrak{R}}{d\theta}\ V\nabla\dot{\mathfrak{R}}\right)\right]dxdydz$$

$$= \frac{1}{8\pi}\iiint\left[\mu(\dot{\xi}^2+\dot{\eta}^2+\dot{\zeta}^2) + 8\pi C\left\{\frac{d\xi}{d\theta}\left(\frac{d\dot{\zeta}}{dy}-\frac{d\dot{\eta}}{dz}\right)\right.\right.$$

$$\left.\left. + \frac{d\eta}{d\theta}\left(\frac{d\dot{\xi}}{dz}-\frac{d\dot{\zeta}}{dx}\right) + \frac{d\zeta}{d\theta}\left(\frac{d\dot{\eta}}{dx}-\frac{d\dot{\xi}}{dy}\right)\right\}\right]dxdydz.$$

Working as before, and obtaining our equations as the conditions that

$$\int(T - W)\,dt$$

should be a minimum, we have to satisfy the equations

$$\iiiint\left[\mu S\dot{\mathfrak{R}}\delta\dot{\mathfrak{R}} + 4\pi CS\left(\frac{d\delta\mathfrak{R}}{d\theta}.V\nabla\dot{\mathfrak{R}}\right) + 4\pi CS\left(\frac{d\mathfrak{R}}{d\theta}.V\nabla\delta\dot{\mathfrak{R}}\right)\right.$$

$$\left. - \frac{1}{K}S(V\nabla\mathfrak{R}.V\nabla\delta\mathfrak{R})\right]dxdydzdt = 0,$$

or, in Cartesian coordinates,

$$\iiiint\left\{\begin{array}{l}\mu(\dot{\xi}\delta\dot{\xi}+\dot{\eta}\delta\dot{\eta}+\dot{\zeta}\delta\dot{\zeta}) \\ +4\pi C\left\{\frac{d\delta\xi}{d\theta}\left(\frac{d\dot{\zeta}}{dy}-\frac{d\dot{\eta}}{dz}\right)+\frac{d\delta\eta}{d\theta}\left(\frac{d\dot{\xi}}{dz}-\frac{d\dot{\zeta}}{dx}\right)\right. \\ \qquad\left.+\frac{d\delta\zeta}{d\theta}\left(\frac{d\dot{\eta}}{dx}-\frac{d\dot{\xi}}{dy}\right)\right\} \\ +4\pi C\left\{\frac{d\xi}{d\theta}\left(\frac{d\delta\dot{\zeta}}{dy}-\frac{d\delta\dot{\eta}}{dz}\right)+\frac{d\eta}{d\theta}\left(\frac{d\delta\dot{\xi}}{dz}-\frac{d\delta\dot{\zeta}}{dx}\right)\right. \\ \qquad\left.+\frac{d\zeta}{d\theta}\left(\frac{d\delta\dot{\eta}}{dx}-\frac{d\delta\dot{\xi}}{dy}\right)\right\} \\ -\frac{1}{K}\left\{\left(\frac{d\delta\zeta}{dy}-\frac{d\delta\eta}{dz}\right)\left(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\right)\right. \\ \qquad+\left(\frac{d\delta\xi}{dz}-\frac{d\delta\zeta}{dx}\right)\left(\frac{\delta\xi}{dz}-\frac{\delta\zeta}{dx}\right) \\ \qquad\left.+\left(\frac{d\delta\eta}{dx}-\frac{d\delta\xi}{dy}\right)\left(\frac{d\eta}{dx}-\frac{d\xi}{dy}\right)\right\}\end{array}\right\}dxdydzdt = 0.$$

Now of course the terms depending on C are the only additional ones, and it will be necessary to integrate them both relatively to the time and also relatively to θ. The integration with respect to the time is easily performed, as it merely consists in removing the dot from one of the terms under the integral to the other and changing the sign, so that, neglecting the terms depending on the limits of the time with which we are not concerned, our equation becomes

$$\iiiint\Big[\mu S(\ddot{\Re}\delta\Re) - 4\pi CS\frac{d\delta\Re}{d\theta}\,V\nabla\dot{\Re} + 4\pi CS\left(\frac{d\Re}{d\theta}.\,V\nabla\delta\Re\right) + \frac{1}{K}S(V\nabla\Re\,.\,V\nabla\delta\Re)\Big]\,dxdydzdt = 0,$$

or, in Cartesian coordinates,

$$\iiiint\left\{\begin{aligned}
&\mu(\ddot{\xi}\delta\xi + \ddot{\eta}\delta\eta + \ddot{\zeta}\delta\zeta)\\
&- 4\pi C\left\{\frac{d\delta\xi}{\delta\theta}\left(\frac{d\dot{\zeta}}{dy} - \frac{d\dot{\eta}}{dz}\right) + \frac{d\delta\eta}{d\theta}\left(\frac{d\dot{\xi}}{dz} - \frac{d\dot{\zeta}}{dx}\right) + \frac{d\delta\zeta}{d\theta}\left(\frac{d\dot{\eta}}{dx} - \frac{d\dot{\xi}}{dy}\right)\right\}\\
&+ 4\pi C\left\{\frac{d\dot{\xi}}{d\theta}\left(\frac{d\delta\zeta}{dy} - \frac{d\delta\eta}{dz}\right) + \frac{d\dot{\eta}}{d\theta}\left(\frac{d\delta\xi}{dz} - \frac{d\delta\zeta}{dx}\right) + \frac{d\dot{\zeta}}{d\theta}\left(\frac{d\delta\eta}{dx} - \frac{d\delta\xi}{dy}\right)\right\}\\
&+ \frac{1}{K}\left\{\left(\frac{d\delta\zeta}{dy} - \frac{d\delta\eta}{dz}\right)\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right) + \left(\frac{d\delta\xi}{dz} - \frac{d\delta\zeta}{dx}\right)\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + \left(\frac{d\delta\eta}{dx} - \frac{d\delta\xi}{dy}\right)\left(\frac{d\eta}{dx} - \frac{d\xi}{dy}\right)\right\}
\end{aligned}\right\} dxdydzdt = 0.$$

Integrating these now by parts relatively to x, y, z, and calling $l : m : n$ the direction cosines of $\Re$ the normal to the

surface of separation, and $al + \beta m + \gamma n = \mathrm{S}\mathfrak{M}\mathfrak{R} = e$, when $\mathfrak{M}$ is the vortex whose components are a, β, γ, we get

$$\int\Big[\iiint\Big\{\mathrm{S}\Big[4\pi\mathrm{C}\Big\{\mathrm{V}\Big(\mathfrak{R}.\frac{d\dot{\mathfrak{R}}}{d\theta}\Big)-\epsilon\mathrm{V}\nabla\dot{\mathfrak{R}}\Big\}+\frac{1}{\mathrm{K}}\mathrm{V}(\mathfrak{R}\mathrm{V}\nabla\mathfrak{R})\Big]\delta\mathfrak{R}\Big\}\,ds$$

$$+\iiint\mathrm{S}\Big\{\Big(\mu\ddot{\mathfrak{R}}+8\pi\mathrm{C}\mathrm{V}\nabla\frac{d\dot{\mathfrak{R}}}{d\theta}+\frac{1}{\mathrm{K}}\mathrm{V}\nabla\mathrm{V}\nabla\mathfrak{R}\Big)\delta\mathfrak{R}\Big\}\,dxdydz\Big]dt=0,$$

or, in Cartesian coordinates,

$$\int\Bigg\{\begin{aligned}
&\iint\frac{1}{\mathrm{K}}\Big[\Big\{n\Big(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\Big)-m\Big(\frac{d\eta}{dx}-\frac{d\xi}{dy}\Big)\Big\}\delta\xi\\
&\qquad+\Big\{l\Big(\frac{d\eta}{dx}-\frac{d\xi}{dy}\Big)-n\Big(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\Big)\Big\}\delta\eta\\
&\qquad\qquad+\Big\{m\Big(\frac{d\xi}{dy}-\frac{d\eta}{dx}\Big)-l\Big(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\Big)\Big\}\delta\zeta\Big]ds\\
&+4\pi\mathrm{C}\iint\Big\{\frac{d}{d\theta}(n\dot{\eta}-m\dot{\zeta})\delta\xi+\frac{d}{d\theta}(l\dot{\zeta}-n\dot{\xi})\delta\eta\\
&\qquad\qquad+\frac{d}{d\theta}(m\dot{\xi}-l\dot{\eta})\delta\zeta\Big\}ds\\
&-4\pi\mathrm{C}\iint(la+m\beta+n\gamma)\Big\{\Big(\frac{d\dot{\zeta}}{dy}-\frac{d\dot{\eta}}{dz}\Big)\delta\xi\\
&\qquad+\Big(\frac{\delta\dot{\xi}}{dz}-\frac{d\dot{\zeta}}{dx}\Big)\delta\eta+\Big(\frac{d\dot{\eta}}{dx}-\frac{d\dot{\xi}}{dy}\Big)\delta\zeta\Big\}ds\\
&+\iiint\mu(\ddot{\xi}\delta\xi+\ddot{\eta}\delta\eta+\ddot{\zeta}\delta\zeta)\,dxdydz\\
&+8\pi\mathrm{C}\iiint\Big\{\frac{d}{d\theta}\Big(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\Big)\delta\xi+\frac{d}{d\theta}\Big(\frac{d\dot{\xi}}{dz}-\frac{d\dot{\zeta}}{dx}\Big)\delta\eta\\
&\qquad+\frac{d}{d\theta}\Big(\frac{d\dot{\eta}}{dx}-\frac{d\dot{\xi}}{dy}\Big)\delta\zeta\Big\}\,dxdydz\\
&+\frac{1}{\mathrm{K}}\iiint\Big\{\Big[\frac{d}{dy}\Big(\frac{d\eta}{dx}-\frac{d\xi}{dy}\Big)-\frac{d}{dz}\Big(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\Big)\Big]\delta\xi\\
&\qquad+\Big[\frac{d}{dz}\Big(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\Big)-\frac{d}{dx}\Big(\frac{d\eta}{dx}-\frac{d\xi}{dy}\Big)\Big]\delta\eta\\
&\qquad+\Big[\frac{d}{dx}\Big(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\Big)-\frac{d}{dy}\Big(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\Big)\Big]\delta\zeta\Big\}\,dxdydz
\end{aligned}\Bigg\}dt=0.$$

I shall not consider the general equations of wave-propagation in the Cartesian form, as they are the same as those given by Maxwell in a more general form, for he assumes that the static energy of the medium is expressed more generally than I have assumed; but as the Quaternion investigation is not long I shall give it.

The equation of motion is

$$\mu\ddot{\mathfrak{R}} + 8\pi C V\nabla \frac{d\dot{\mathfrak{R}}}{d\theta} + \frac{1}{K} V\nabla V\nabla V\nabla \mathfrak{R} = 0,$$

and if s be the normal to a wave-plane, we may evidently assume $\nabla = k\frac{d}{ds}$, and as $\mathfrak{R}$ is in the wave-plane, we have

$$\mathfrak{R} = ai \sin \frac{2\pi}{\lambda}(vt - s) + bj \cos \frac{2\pi}{\lambda}(vt - s),$$

and as $\frac{d}{d\theta} = \gamma \frac{d}{dz}$ our equation becomes

$$\mu\ddot{\mathfrak{R}} + 8\pi C\gamma V k \frac{d^3\mathfrak{R}}{dz^2 dt} + \frac{1}{K} V k \frac{d}{dz} V k \frac{d\mathfrak{R}}{dz} = 0.$$

Substituting for $\mathfrak{R}$, and equating the coefficients of i and j separately to zero, we get

$$a\mu v^2 + \frac{16\pi^2 C\gamma}{\lambda^2} bv - \frac{a}{K} = 0, \quad b\mu v^2 + \frac{16\pi^2 C\gamma}{\lambda^2} av - \frac{b}{K} = 0,$$

from which it is easy to see that $a = \pm b$, and that v is determined by the equation

$$\mu v^2 \mp \frac{16\pi^2 C\gamma}{\lambda^2} v - \frac{1}{K} = 0,$$

which gives of course two different values of v, one for each circularly polarized ray, or disregarding the solutions for waves going in the negative direction, we have approximately

$$v_1 = \frac{1}{\sqrt{\mu K}} + \frac{8\pi^2 C\gamma}{\mu\lambda^2}, \quad v_2 = \frac{1}{\sqrt{\mu K}} - \frac{8\pi^2 C\gamma}{\mu\lambda^2},$$

and of course C can easily be determined from these by observing

the rotation produced, but there is nothing except experiment to prove that C may not be a function of λ, as we know K to be, to some extent at least; so that using these formulæ in order to obtain the laws of dispersion of rotatory polarization seems to approach towards deducing the known from the unknown. All we can be sure of is that C is in general extremely minute.

Returning to the superficial equations, and using / as a sign of substitution, we get

$$/\frac{1}{K}\mathrm{V}(\mathfrak{R}\,.\,V\nabla\mathfrak{R}) + 4\pi\mathrm{C}\left(\mathrm{V}\mathfrak{R}\frac{d\dot{\mathfrak{R}}}{d\theta} - \varepsilon\mathrm{V}\nabla\dot{\mathfrak{R}}\right) = 0.$$

As I intend only to work out the results in Cartesian coordinates, I shall confine myself to them, and for simplification assume the axes to be z normal to and x and y in the surface, and consequently $l = m = 0$, $n = 1$, and $\varepsilon = \gamma$. C may also be supposed to vanish for one of the media for which K_1 is the dielectric inductive capacity. I shall also assume that $\delta\zeta = 0$ as the vanishing of its coefficient leads to inconvenient results, and the assumption may be to some extent justified by considering that as these $\delta\xi$, $\delta\eta$, $\delta\zeta$ are superficial values, no virtual displacement out of the surface, as this would be, is admissible. From the other two, $\delta\xi$ and $\delta\eta$, we evidently get

$$\frac{1}{K_1}\left(\frac{d\xi_1}{dz} - \frac{d\zeta_1}{dx}\right) = \frac{1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + 4\pi\mathrm{C}\frac{d\dot{\eta}}{d\theta} - 4\pi\mathrm{C}\gamma\left(\frac{d\dot{\zeta}}{dy} - \frac{d\dot{\eta}}{dz}\right),$$

$$\frac{1}{K_1}\left(\frac{d\zeta_1}{dy} - \frac{d\eta_1}{dz}\right) = \frac{1}{K}\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right) + 4\pi\mathrm{C}\frac{d\dot{\xi}}{d\theta} + 4\pi\mathrm{C}\gamma\left(\frac{d\dot{\xi}}{dz} - \frac{d\dot{\zeta}}{dx}\right).$$

A remarkable point about these equations is that they admit of being integrated with regard to the time as far as their right-hand members are concerned, so that, in addition to the values which satisfy them as they stand, and which are the only ones of much interest, there are other periodic values of ξ, η, ζ independently of ξ_1, η_1, ζ_1, which satisfy these superficial conditions, and which may consequently be looked upon as a sort

of free vibration of the surface of the medium.* But even if this could be propagated into the rest of the ether it is improbable that the resultant vibrations would be of such a period as to be visible, though some energy might be expended on them.

I shall now further assume that the axis of x is the intersection of the surface with the plane of incidence of a plane wave, and that consequently none of my quantities are functions of y, which reduces these equations to

$$\frac{1}{K_1}\left(\frac{d\xi_1}{dz} - \frac{d\zeta_1}{dx}\right) = \frac{1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + 4\pi C\left(\frac{d\eta}{d\theta} + \gamma\frac{d\eta}{dz}\right),$$

$$\frac{1}{K_1}\frac{d\eta_1}{dz} = \frac{1}{K}\frac{d\eta}{dz} - 4\pi C\left\{\frac{d\xi}{d\theta} + \gamma\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right)\right\}.$$

In order still further to simplify the problem I shall now consider separately the cases in which the magnetization of the medium is normal and that in which it is in the surface. In the first place, it is evident that if it were all in y we should have $\alpha = \gamma = 0$, $\beta = \mathfrak{M}$, and the coefficient of C would vanish in both equations, because $\frac{d}{d\theta} = \mathfrak{M}\frac{d}{d\eta}$, and hence we get Mr. Kerr's result (Phil. Mag., March, 1878, p. 174) that when the plane of incidence is normal to the lines of magnetic force the magnetizing of the mirror produces no change in the reflected light.†

[* This seems to be the incipient compressile wave introduced in footnote, p. 66.]

[† Observe, however, that the velocity of propagation v_1, v_2, or s, and therefore also the angle of refraction r, depends on C; so that, although there is no magneto-optic perpendicular component added to the vibration, there is change of amplitude, and also of phase in metallic reflection, produced by the magnetic field. These effects have been calculated for this case by Wind, and by Leathem (*loc. cit.* p. 66), and their theoretical order of magnitude verified experimentally by Zeeman.

As the velocity of propagation is changed in opposite directions, to v_1 and v_2, for the two types of circularly polarized waves, these must be treated separately in the analysis, instead of taking a plane-polarized wave as in what follows in the text. Thus the next two results, those for the case when the magnetization is normal to the surface, require alteration. But in the more important case of tangential magnetization, which is the one afterwards utilized (p. 71) to compare with Kerr's

Assuming, then, first that the magnetization is normal to the surface, we have

$$\alpha = \beta = 0, \quad \gamma = \mathfrak{M}, \quad \text{and} \quad \frac{d}{d\theta} = \mathfrak{M}\frac{d}{ds},$$

so that, calling $4\pi CK_1\mathfrak{M} = \nu$, our equations become

$$\left.\begin{aligned} \frac{d\xi_1}{dz} - \frac{d\zeta_1}{dx} &= \frac{K_1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + 2\nu\frac{d\eta}{dz} \\ \frac{d\eta_1}{dz} &= \frac{K_1}{K}\frac{d\eta}{dz} - \nu\left(2\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) \end{aligned}\right\}, \quad \ldots\ldots \text{(I)}$$

while if it be all in x—*i.e.* in the intersection of the plane of incidence and the surface,

$$\alpha = \mathfrak{M}, \quad \beta = \gamma = 0, \quad \text{and} \quad \frac{d}{d\theta} = \mathfrak{M}\frac{d}{dx},$$

so that our equations are

$$\left.\begin{aligned} \frac{d\xi_1}{dz} - \frac{d\zeta_1}{dx} &= \frac{K_1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + \nu\frac{d\eta}{dx} \\ \frac{d\eta_1}{dz} &= \frac{K_1}{K}\frac{d\eta}{dz} - \nu\frac{d\xi}{dx} \end{aligned}\right\}, \quad \ldots\ldots \text{(II)}$$

I shall now proceed to solve these two systems of equations each for the two cases of waves whose magnetic forces ξ, η, ζ are first in the plane of incidence, and secondly at right angles to it. From the forms of the equations it is evident that we cannot assume either the reflected or refracted rays to be similarly polarized, but it is easy to convince oneself that there can be no difference of phase introduced in this case any more than in the former one of ordinary reflection; and, as I then remarked, it is evidently a question of greater complication than to be capable of being deduced from the simple assumption that the alteration of the nature of the medium in going from one into another is abrupt. Until more is known of the nature and extent of this change I fear we must be content with theories which only partially represent the facts.

experiments, the result comes out (p. 69) identical for both states of polarization of the ray and is thus not affected.

The extension of all this analysis to metallic reflection is made by simply taking the various physical constants to be complex quantities.]

As before, I shall assume ξ_1, η_1, ζ_1 to be due to the incident and reflected rays, while ξ, η, ζ are due to the refracted ray. The incident ray may be taken to depend upon the angle

$$\phi_1 = \frac{2\pi}{\lambda_1}(t - z\cos i - x\sin i),$$

calling the velocity in this medium unity, and the reflected wave on

$$\phi'_1 = \frac{2\pi}{\lambda_1}(t + z\cos i - x\sin i),$$

as it is easy to see that for these to vanish from the equations when $z = 0$ we must have the sines of the angles of incidence and reflection equal; while the refracted wave must be taken to depend upon

$$\phi = \frac{2\pi}{\lambda}(st - z\cos r - x\sin r),$$

and here we must have, as before,

$$\frac{s}{\lambda} = \frac{1}{\lambda_1}, \quad \frac{\sin i}{\lambda_1} = \frac{\sin r}{\lambda}$$

and this involves, as before, the ordinary laws of refraction, as s is constant, and, in general,

$$s = \sqrt{\frac{\mu_1 K_1}{\mu K}}, \text{ therefore } \frac{\mu_1 K_1}{\mu K} = s^2 = \frac{\sin^2 r}{\sin^2 i}.$$

Taking then first the case of the magnetization being all normal to the surface and the incident displacements ξ, η, ζ in the plane of incidence, we must evidently assume

$$\left.\begin{aligned} \xi_1 &= a_1\cos i\cos\phi_1 - b_1\cos i\cos\phi'_1, & \xi &= a\cos r\cos\phi, \\ \eta_1 &= c_1\sin\phi'_1, & \eta &= c\sin\phi, \\ \zeta_1 &= -a_1\sin i\cos\phi_1 - b_1\sin i\cos\phi'_1, & \zeta &= -a\sin r\cos\phi, \end{aligned}\right\} \quad \text{(A)}$$

in order to satisfy the equations

$$\frac{d\xi_1}{dx} - \frac{d\zeta_1}{dx} = \frac{K_1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + 2\nu\frac{d\eta}{dx},$$

$$\frac{d\eta_1}{dx} = \frac{K_1}{K}\frac{d\eta}{dx} - \nu\left(2\frac{d\xi}{dx} - \frac{d\zeta}{dx}\right),$$

and $$\xi_1 = \xi, \qquad \eta_1 = \eta.$$

As it leads to inconsistent results I, in accordance with Fresnel, omit the third equation, $\zeta_1 = \zeta$. It is not easy to justify this omission, I fear, and the result must do so, as well as the consideration that probably if we had a better insight into the nature of the change from one medium into another our equations would be so modified as not to present these anomalies.*

[* The explanation of the inconsistency lies in the assumption that there is no tendency to produce waves of other than transverse type in ξ, η, ζ. The equations of propagation lead to

$$\frac{d^2}{dt^2}\left(\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz}\right) = 0,$$

so that no wave of compression can travel in an infinitely extended *uniform* medium. But in crossing an interface where the uniformity is broken there may be a tendency to initiate compressional disturbance, which must call into play an opposing internal stress in the media. In magnetized media the discrepancy in the text shows that, on the theory there adopted, such a tendency exists. The nullity of the compression must thus be explicitly introduced as a condition of constraint into the process of variation of the Action, leading to a further modification of the equations of propagation by the presence of this adjusting internal compressile stress. If, on the other hand, we desire to retain the equations of the text, Maxwell's magneto-optic energy term must be dropped, and instead the form of the function U, which expresses the potential energy of the medium in terms of (ξ, η, ζ), must be modified by inclusion of differential coefficients of the latter. In the magnetic problem these are with respect to time, thus indicating a kinetic origin concerned with the exciting magnetic field; in the problem of quartz they are spacial, and so indicate an intrinsic structural origin. (See Larmor, "On the Action of Magnetism on Light," *Brit. Assoc. Report*, 1893.) It has been proved by Leathem, *Trans. Camb. Phil. Soc.*, 1897, that the former type of theory is inconsistent with experiment. The latter type conserves the analytical solution developed in the text; the investigation has been extended to metallic media by various writers, but under this logical form and most completely by Leathem, *Phil. Trans.*, 1897, and by Wind, *Archives néerlandaises*, 1897, or *Physical Review*, vol. vi, and has been shown to afford a good correlation of the great mass of experimental measurements which have been accumulated in the various branches of this subject.]

From the last equation it is manifest that $c = c_1$, and as ν is very small it is obvious that c will always be small, so that $\nu\eta$ may be omitted in the first equation. Putting in the values of ξ, η, ζ, &c., and remembering that when $z = 0$, $\phi_1 = \phi'_1 = \phi$, these equations evidently reduce to

$$(a_1 + b_1)\sin i = \frac{\mu}{\mu_1} a \sin r,$$

$$c_1 \sin i \cos i = -\frac{\mu}{\mu_1} c_1 \sin r \cos r - \frac{2\pi\nu}{\lambda} a(1 + \cos^2 r)\sin i,$$

$$(a_1 - b_1)\cos i = a \cos r;$$

and from these we must solve for a, b, and c in terms of a_1.

However, c_1, the component introduced at right angles to the original plane of vibration by reflection, is the only one of much interest, though the alteration produced in the values of the reflected component by assuming $\frac{\mu}{\mu_1}$ to differ from unity are noteworthy. Calling T the period of vibration of the wave we are considering, we may put $\frac{s}{\lambda} = \frac{1}{T}$, and if we call $\frac{\mu}{\mu_1} = \chi$, we get for c the equation

$$c_1 = -\frac{4\pi\nu}{sT} a_1 \frac{(1 + \cos^2 r)\sin i \sin 2i}{(\sin 2i + \chi \sin 2r)(\sin i \cos r + \chi \cos i \sin r)},$$

which, if $\chi = 1$, reduces to

$$c_1 = -\frac{4\pi\nu}{T} a_1 \frac{(1 + \cos^2 r)\sin^2 i \cos i}{\sin^2(i + r)\cos(i - r)\sin r}.$$

In the second case, when the direction of the vibration of the incident ray is perpendicular to the plane of incidence, we must evidently assume

$$\left.\begin{aligned} \xi_1 &= -c_1 \cos i \sin \phi'_1, & \xi &= c \cos r \sin \phi, \\ \eta_1 &= a_1 \cos \phi_1 + b_1 \cos \phi'_1, & \eta &= a \cos \phi, \\ \zeta_1 &= -c_1 \sin i \sin \phi'_1, & \zeta &= -c \sin r \sin \phi, \end{aligned}\right\} \quad \ldots \quad (B)$$

and, as before, it is evident that c is generally very small, so that $\nu\xi$ and $\nu\zeta$ may be omitted, and our equations become

$$\frac{d\xi_1}{dz} - \frac{d\zeta_1}{dx} = \frac{K_1}{K}\left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right) + 2\nu\frac{d\eta}{dz},$$

$$\frac{d\eta_1}{dz} = \frac{K_1}{K}\frac{d\eta}{dz},$$

$$\xi_1 = \xi, \qquad \eta_1 = \eta;$$

and when the values above are introduced into them we obtain

$$-c_1\cos i = c\cos r,$$

$$(a_1 - b_1)\sin i\cos i = a\chi\sin r\cos r,$$

$$a_1 + b_1 = a,$$

$$-c_1 = -\chi\frac{\sin r}{\sin i}c + \frac{4\pi\nu}{T}a\frac{\sin i\cos r}{\sin r}.$$

Hence we get, solving for c_1 in terms of a_1,

$$c_1 = -\frac{8\pi\nu}{T}a_1\frac{\sin 2i\sin^2 i\cos^2 r}{\sin r(\sin 2i + \chi\sin 2r)(\sin i\cos r + \chi\sin r\cos i)},$$

which, when $\chi = 1$, reduces to

$$c_1 = -\frac{4\pi\nu}{T}a_1\frac{\sin 2i\sin^2 i\cos^2 r}{\sin r\sin^2(i+r)\cos(i-r)}.$$

Turning now to the case where the magnetization is in the surface, I shall first suppose the incident vibration to be in the plane of incidence, when we evidently assume, as before, equations (A) for $\xi_1, \eta_1, \zeta_1, \xi, \eta, \zeta$; and these must satisfy the equations (II), and in addition

$$\xi_1 = \xi, \quad \eta_1 = \eta.$$

As before, we evidently get $c = c_1$, and from the second equation of (II) see that c must be small, as ν is, and consequently we may omit $\nu\eta$ in the first equation, and so the first equation is the same as before, and we have

$$(a_1 + b_1)\sin i = a\chi\sin r,$$

$$(a_1 - b_1)\cos i = a\cos r,$$

$$c_1\sin i\cos i = -c_1\chi\sin r\cos r - \frac{2\pi\nu}{T}a\cos r\sin^2 i,$$

which, when solved for c_1 in terms of a_1, gives

$$c_1 = -\frac{4\pi\nu}{T} a_1 \frac{\sin 2i \sin^2 i \cos r}{(\sin 2i + \chi \sin 2r)(\sin i \cos r + \chi \cos i \sin r)},$$

which, when $\chi = 1$, reduces to

$$c_1 = -\frac{2\pi\nu}{T} a_1 \frac{\sin 2i \sin^2 i \cos r}{\sin^2(i+r)\cos(i-r)}.$$

Finally, supposing the incident vibration to be perpendicular to the plane of incidence, we have ξ_1, η_1, ζ_1, ξ, η, ζ determined by the equation (B), which, when substituted in equations (II), neglecting $\nu\xi$ as before on account of its smallness, give

$$c_1 = -\frac{4\pi\nu}{T} a_1 \frac{\sin 2i \sin^2 i \cos r}{(\sin 2i + \chi \sin 2r)(\sin i \cos r + \chi \cos i \sin r)},$$

which, when $\chi = 1$, reduces to

$$c_1 = -\frac{2\pi\nu}{T} a_1 \frac{\sin 2i \sin^2 i \cos r}{\sin^2(i+r)\cos(i-r)}.$$

It is remarkable that in this case the two components introduced by reflection are the same, whether the vibration be in, or perpendicular to, the plane of incidence.

Comparing this method of obtaining the effects of reflection from a magnetized surface with that given by me in the Proceedings of the Royal Society for 1876, No. 176, it is to be observed that my equations (I) and (II) are unaltered if the signs of ν and η are both reversed together, or if those of ν and ξ and ζ are all reversed, showing that a circularly polarized ray in one direction is reflected according to the same laws when the magnetization is one way as the oppositely circularly polarized ray would be if the magnetization were reversed, and hence my former method of dividing the incident plane polarized ray into two opposite circularly polarized ones, each of which was reflected according to its own laws, is justified.

In comparing these expressions with the results of Mr. Kerr's admirable experiments, it is necessary to observe, as I mentioned

before, that the introduction of a difference of phase between the reflected components is a question of a different order from that here discussed, and probably to some extent at least depends on the want of abruptness in the change from one medium to the other. For instance, my expressions give no change of plane of polarization when light is reflected normally from the end of a magnet, but they would lead one to expect that the only effect was a slight elliptic polarization, the major axis of the ellipse being in the same plane as the original plane of polarization. Now Mr. Kerr's experiments show that there is some rotation of this plane by reflection, and a supposition similar to one long ago proposed to explain the known elliptic polarization of metallic reflection—namely, that the efficient reflecting surface has some depth—may easily be shown to lead to Mr. Kerr's result. On this hypothesis the reflected ray is the resultant of the rays reflected from a small thickness at the surface of separation of the media; and in the case of normal reflection from the end of the pole of a magnet, each of these components would be slightly turned from its original plane of polarization owing to having passed through a very small thickness of a very powerful rotatory polarizing substance—namely, this superficial layer of the magnet—hence it is evident that their resultant would no longer be polarized in the same plane as the incident ray. I only give this as an instance of how this question of a difference of phase affects the results, and how the hypotheses that have been framed to explain it might be used to bring my results into complete accord with Mr. Kerr's experiments. I hardly think it worth while going into this more fully, as it is treading so closely upon unknown ground—namely, the connexion between matter and ether—that our hypotheses are to a great extent merely conveniences.

Another question is the extent to which χ affects ordinary reflection from a powerfully magnetic substance like iron. I have never come across any experiment tending to show that the reflection from iron was at all peculiar. This may be owing to the electrostatic inductive capacity being a characteristic of

the ether in the matter, while magnetic inductive capacity is a characteristic of the matter, and so only affects the wave-propagation in a secondary degree—*i. e.*, only to the extent to which ether motions are transformed into currents in or motions of the matter. The same applies to the opacity of the medium, which is similarly due to an exchange of energy between the ether and the matter. The function Professor Maxwell assumes to represent the effect of magnetization on wave-propagation is an expression of the same hypothesis. In comparing my equations with Mr. Kerr's results I shall consequently assume $\chi = 1$.

Mr. Kerr's most elaborate experiments have been made with the lines of magnetic force in the intersection of the surface of the medium and the plane of incidence,* and I shall confine my attention to this case. My equations give that the principal effect of reflection is to introduce a component perpendicular to the original plane of polarization whose amplitude is represented by the equation

$$c = -\frac{2\pi\nu}{T}\,\frac{\sin 2i \sin^2 i \cos r}{\sin^2(i+r)\cos(i-r)}$$

when the amplitude of the incident vibration is unity.

The following table represents the values of the variable part of this for the several incidences mentioned by Mr. Kerr in his paper. This table is calculated on the assumption that 75° is the polarising angle for iron.

$i =$	0	30°	45°	60°	65°	75°	80°	85°	90°
$\frac{\sin 2i \sin^2 i \cos r}{\sin^2(i+r)\cos(i-r)} =$	0	·6272	·8646	1·0007	1·0058	·9000	·7554	·4904	0

This table shows that the intensity of this introduced component vanishes for the two limiting incidences 0° and 90°, and attains a maximum value of 1·0067 at about 63° 20′. On comparing this with Mr. Kerr's results there is a most striking

* Phil. Mag., March, 1878.

correspondence. Summing up the results of his paper on reflection from a surface which is magnetized so that the lines of force are in the surface and the plane of incidence (Phil. Mag., March, 1878, § 23), he says: "When the vibration reflected from the unmagnetized mirror is either parallel or perpendicular to the plane of reflection, the effect of magnetization is to introduce a new and very small component vibration in a direction perpendicular to the primitive vibration." This is what I have called *c*. He goes on to define its direction relatively to the Amperean currents which are supposed to produce the magnetic force; but as the sign of *c* depends on the sign of ν, and thus on that of C, which cannot be certainly determined for iron otherwise than by these very experiments, any confirmation founded upon it such as I mentioned in my former paper is to a certain extent illusory; but the same arguments as are there used would lead to the same conclusion here—namely, that iron, if transparent, would behave like those ferromagnetic substances observed by Verdet, which rotate the plane of polarisation in the opposite direction to the Amperean currents, as is of course most probable. He further mentions, in § 24, that the phase of this component is always nearly the same as that of the component of the reflected ray polarized in the plane of incidence. This question, however, of difference of phase is one I am not at present prepared to explain, and, as I have mentioned already, must await a more comprehensive theory. Considering next the intensity, of course no very accurate measures were possible on account of the minuteness of the phenomenon; but he several times remarks that all effects of magnetization vanished at normal and grazing instances, while his maximum effects were always obtained at incidences of about 60° or 65°. The anomalies observed connected with the incidence 75° all belong to the question of the difference of phase between the components, which my investigation does not touch. On the whole then I think that my results, as far as they go, are in complete accordance with Mr. Kerr's experiments.

This investigation is put forward as a confirmation of Professor Maxwell's electromagnetic theory of light, in which, though there are some points requiring further investigation, nevertheless the foundation has certainly been laid of a very great addition to our knowledge, and if it induced us to emancipate our minds from the thraldom of a material ether might possibly lead to most important results in the theoretic explanation of nature.

9

NOTE ON SURFACE TENSION

[From the *British Association Report*, 1878. See No. 6 *supra*.]

ASSUMING that the constant difference of electric potential at the junction of two dissimilar fluids is due to a tendency towards some chemical combination of the two, it is easy to see that if χ be the electrical charge corresponding to each electrochemical equivalent, which is the same for all bodies, and if n be the number of such equivalents in presence of one another per unit of surface, and if ϵ be the difference of potential of contact, then there is a potential energy per unit of surface $= n\chi\epsilon$, and this must be the superficial tension if it be all due to this cause, as M. Lippmann's recent experiments seem to show. Hence

$$T = n\chi\epsilon.$$

If ϵ be assumed about ·1 of a volt, and $T = 0{\cdot}8$ grammes per centimetre, as it is in the case of water, it is possible to calculate the thickness of the superficial layer which contains molecules which act upon one another, and the result is

$$\theta = 7{\cdot}8 \times 10^{-8} \text{ of a centimetre,}$$

which approximates towards the quantities obtained by other methods.

10

ON THE THEORY OF MUSCULAR CONTRACTION

[From the *British Association Report*, 1878. See No. 6 *supra*.]

ASSUMING a muscle to consist of fibriles averaging $\frac{1}{8000}$th of a centimetre in diameter, and that they may be treated as a system of elongated cylinders of fluid with a superficial tension capable of variation by the action of nervous stimuli in accordance with M. Lippmann's experiments on the relations of electrical difference of potential to the superficial tension of fluids, it may be shown that a force of four kilogrammes per square centimetre might be produced, which approximates to the observed maximum contractile force. The striations in striated fibre are accounted for by the instability of uniform fluid cylinders; the necessity for a continual stimulus to keep up a given contraction by the constant repairs going on in living muscle; and the heating of a muscle when it contracts is completely explained by the fact that all fluid surfaces heat when they are allowed to contract under the action of their superficial tension.

11

ON THE SUPPORT OF SPHEROIDAL DROPS AND ALLIED PHENOMENA*

[From the *British Association Report*, 1878. See Nos. 4, 5 *supra*.]

THE authors give an account of several investigations and experiments† made by them during the past year upon the unequal stresses which arise in gas when polarized, that is, in which the molecular motions are not alike in all directions, and especially in the case where the polarization arises from heat traversing the gas, as within radiometers, and in the gaseous layers which support spheroidal drops, those which protect the hand from a dangerous burn when dipped into melted metal, and many others. In such cases the molecular motions are not alike in all directions, and cause the stress within the gas directed across the layer to exceed the transverse stresses by an amount which may be called the Crookes's, or polarization, stress.

The investigation of this stress by a direct mechanical consideration of the molecular motions had been much facilitated by employing the conception of a tube placed across the layer,

* Conjointly with G. Johnstone Stoney, M.A., F.R.S. and Richard J. Moss, Keeper of the Minerals in the Museum of Science and Art, Dublin.

† See the following memoirs:—"On some Remarkable Instances of Compressed Strata of Polarized Gas at Ordinary Atmospheric Tensions," by G. Johnstone Stoney (Scientific Proceedings of the Royal Dublin Society, vol. i, p. 53; Phil. Mag., June 1878, p. 457). "On the Spheroidal State," by Richard J. Moss (Scientific Proceedings of the Royal Dublin Society, vol. i, p. 83). "On the Mechanical Theory of Polarization Stress in Gases," by G. Johnstone Stoney (Scientific Transactions of the Royal Dublin Society, vol i, memoir 5). "On the Mechanical Theory of Crookes's Force," by George F. Fitz Gerald (Scientific Transactions of the Royal Dublin Society, vol. i, memoir 6).

closed at the ends by patches of the heater and cooler, and with sides that reflect molecules by the same law that a polished surface reflects light. By employing this conception, Mr. Stoney had shown that for small differences of temperature, and with a heater and cooler that are large, flat, parallel, and at a fixed distance asunder, the polarization stress will vary nearly as

$$\frac{G^2}{PT},$$

where G is the flow of heat, P the tension of the gas, and T its temperature from absolute zero.

By a wholly different method, first suggested by Mr. Fitz-Gerald in a letter to *Nature*, which consists in calculating the transverse stress in the same way as Clausius has calculated the stress across the layer, Mr. Stoney had obtained the following symbolical expression for the difference between these stresses, that is, for the polarization stress:

$$\tfrac{1}{4}P\int_{-1}^{+1} IV^2(3\mu^2 - 1)\,d\mu,$$

where μ is the cosine of the angle made by the path of a molecule with the normal to the layer, I the proportion of molecules moving at that angle, and V^2 the mean of the squares of their velocities.

By applying the same method of investigation to the general case of gas polarized in any way, Mr. FitzGerald had obtained the expressions for the most general stresses that can arise, radial and tangential, and had shown them to be identical in form with the corresponding equations given by Maxwell in his electromagnetic theory of light.

And, finally, by attending to the conditions indicated by the theory, Mr. Moss had succeeded in keeping spheroidal drops in existence for upwards of an hour and a half, and had ascertained that spheroidal drops may be supported in air devoid of vapour. This last very important confirmation of the theory was established by experiments on melted paraffin, at temperatures at which separate experiments had shown that no evaporation could be detected after a prolonged exposure of the paraffin in vacuo.

12

ON M. DUTER'S EXPERIMENTS ON ELECTRIC ACCUMULATORS

[From the *Scientific Proceedings of the Royal Dublin Society.* Read February 17, 1879.]

M. DUTER has shown by his experiments, as published in the Comptes Rendus, November 1878, vol. 87, p. 828, that when an electric accumulator is charged there is an increase in volume of the dielectric.

I propose to show that a portion, at least, of this effect must be due to the known laws of electric action, but a number of quantitative experiments would be required in order to prove that the whole effect was due to the cause I am about to mention.

In order to simplify the treatment I will take the case of a spherical accumulator of internal radius a, and external b. For the equilibrium of this shell, the pressures inside and out must bear a certain proportion to one another if it is to remain undeformed. If we consider the equilibrium of the semi-shell supporting inside a pressure P, and outside p, while along the edge there is a tension T, we must have

$$Pa^2 = pb^2 + T(b^2 - a^2),$$

showing that unless P and p are to one another inversely as the areas on which they act, there must be either enlargement or contraction of the shell, as its surface must be either in tension or compression.

Now, the superficial pressures produced when the shell is

made into an electric accumulator are by no means inversely proportional to the areas. If the external surface be at potential zero, we have the following expression for the potential at any point in the dielectric at a distance r from the centre when the quantity of electricity on the inside is Q:—

$$V = Q\left(\frac{1}{r} - \frac{1}{b}\right).$$

Hence the electric force $F = \frac{Q}{r^2}$, and this produces a pressure on the inside

$$P = \frac{F_1^2}{4\pi} = \frac{Q^2}{4\pi a^4}$$

and on the outside

$$p = \frac{F_2^2}{4\pi} = \frac{Q^2}{4\pi b^4}.^*$$

Hence $$Pa^4 = pb^4,$$

or the pressures vary inversely as the *squares* of the areas on which they act.

From this it is easy to calculate the tension in the shell,

$$T = P\frac{a^2}{b^2}.$$

Now, if we call $\theta = b - a$ the thickness of the shell, we get

$$Q = \frac{Vab}{\theta},$$

and consequently $$T = \frac{V^2}{4\pi\theta^2}.$$

So that when charged to a given potential, the tension of the shell varies inversely as the square of its thickness. M. Duter observed (see Comptes Rendus, vol. 87, p. 1036) that the enlargement varied inversely as the square of the thickness of the accumulator, and as the extension of the shell would be generally proportional to its tension, it is reasonable to conclude that the effect M. Duter observed is due to the cause I mention.

[* These pressures should be halved.]

Part of the effect in actual accumulators may be due to deformations of the surface, which tend to make it more spherical. In the case, however, of an ellipsoidal surface, the tensions on the ends are so very much greater than elsewhere that it does not seem likely that it would tend to become more spherical.*

Note.—Mons. D. J. Korteweg, in the Comptes Rendus, No. 7, 17th February, 1879 (the same day as the above communication was read), gives the same explanation as I have given, and, besides, he mentions that he has made experiments showing that it is quantitatively a sufficient one.

[* On this subject of electrostriction, which has been very fully developed in recent years, see Larmor, Phil. Trans. 1877 *A*, pp. 280–4 and 291–6; also Sacerdote, *Journal de Physique*, 1901, which contains a discussion of recent experimental developments and verifications.]

13

ON THE MAXIMUM TENSION OF VAPOURS NEAR CURVED LIQUID SURFACES

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read June 26, 1879.]

IN the year 1870, Sir William Thomson pointed out, in the Proceedings of the Royal Society of Edinburgh, that the maximum tension of vapour near a curved liquid surface must be different from that near a flat one.

I have not come across any reference to an explanation of this fact, and propose the present one, though it is so obvious that I fear it must have been proposed before.

It is generally received that the tension of vapour in contact with its liquid attains equilibrium when the number of molecules emitted by the liquid per unit of time per unit of surface is equal to the number of those admitted similarly. Hence any cause which tends to increase the former and decrease the latter must increase the tension.

In estimating the numbers of molecules emitted from a spherical drop I shall assume that they are emitted from all points of a stratum, of a very small depth, and that the number of those emitted in a particular direction from a point is a function of the distance they have to travel inside the stratum.

Let a be the radius of the drop, and b the length of the radius to the emitting point, which is less than a by the very small quantity t which is the depth of the emitting point below the surface of the drop. Let v be the distance from the end of b to any point on the surface, and let $f(v)$ represent the proportion emitted, which traverse a length v in the stratum; then the

number emitted from the depth t, in the directions between v and $v + dv$, may be written

$$\delta N = \frac{2\pi a}{b} vf(v)dv\,dt.*$$

If we integrate this all round the point we have, calling

$$\int vf(v)dv = n_0, \quad \delta N = \frac{2\pi a}{b} n_0 dt,$$

where of course n_0 is a function of t. It is to be observed that $f(v)$ must be of such a nature as to vanish for all values of v which exceed a very small amount.

Even though $f(v)$ were a function of the angle v makes with the normal to the surface, it is evident that it might be expanded in powers of $\frac{v}{a}$, since v is very small compared with a; and hence the final form of N would be unaltered.

Now $a - b = t$; therefore $\frac{a}{b} = 1 + \frac{t}{a}$, q. p., since a is very large indeed compared with t; so that calling

$$N_0 = 2\pi \int n_0 dt, \quad \text{and} \quad N_0 k = 2\pi \int n_0 t dt,$$

both these integrals being taken between $t = 0$ and $t =$ that small value for which n_0 must vanish, we get

$$N = N_0\left(1 + \frac{k}{a}\right).$$

Similarly, for the particles entering from a small distance outside,

$$N' = N'_0\left(1 - \frac{k}{a}\right).$$

[* It is not obvious how this is obtained. If θ is the angle between v and t, the total number emitted per unit area from the spherical drop seems to be

$$\iint f(v) \cdot 2\pi \sin\theta\, d\theta / 4\pi \cdot 4\pi b^2 db / 4\pi a^2,$$

where $$a^2 = b^2 + v^2 + 2bv\cos\theta;$$

so that it is $$\tfrac{1}{2}\int_0^\infty \left(1 - \frac{t}{a'}\right) dt \int_t^\infty dv\, f(v) \left\{\left(1 - \frac{t}{2a}\right)\frac{t}{v^2} - \frac{1}{2a}\right\}.$$

The last term, depending on the absolute size of the drop, is negligible, so the result becomes

$$\tfrac{1}{2}\int_0^\infty t\left(1 - \frac{3t}{2a}\right) dt \int_0^\infty v^{-2} f(v)\, dv.$$

The correction is thus proportional to the curvature a^{-1}, as might have been assumed *a priori*, and the final formulæ of the text are justified.]

Now, N_0 and N'_0 are obviously the numbers leaving and entering the surface when it is plane. Hence, we get that the number of molecules leaving is greater, and of those entering less, than in the case of a plane surface.

This is obvious geometrically, because to a path of given length in the emitting stratum there corresponds a greater angular aperture for emission in the case of the spherical surface than in that of the plane.

If we now suppose that the number entering the plane surface is proportional to the tension ω_0 of the vapour in contact with it, so that $N'_0 = K_0\omega_0$,

we have $N' = K\omega_0 = K_0\omega_0\left(1 - \frac{k}{a}\right)$; therefore $K = K_0\left(1 - \frac{k}{a}\right)$,

so that if the tension near the bent surface becomes ω the number N′ entering from it becomes $K\omega = K_0\omega\left(1 - \frac{k}{a}\right)$.

This must be equal to N when there is equilibrium; and $N_0 = N'_0$ if ω_0 be the tension corresponding to the flat surface;* so that we get

$$\omega\left(1 - \frac{k}{a}\right) = \omega_0\left(1 + \frac{k}{a}\right),$$

or

$$\omega = \omega_0\left(1 + \frac{2k}{a}\right).$$

Hence, we see that *the maximum tension near a curved surface exceeds that near a plane by a quantity which varies inversely as the radius of curvature of the surface:* and this is the same law as Sir William Thomson arrived at.

In order to utilize this formula for measurements of surface tension it would be necessary to know something about the law of emission and rates of evaporation. It might be possible to determine something about the latter by observing under a microscope the rates of evaporation of drops of various sizes, and by that means estimating k.

[* The procedure is, the output and intake involve factors depending on the curvature, which are determined by molecular considerations: the intake is also proportional to the vapour tension: equating intake to output at the curved surface with its modified vapour tension, and also at a flat surface with the normal tension, we obtain sufficient equations.]

14

NOTE ON THE CONDUCTIVITY OF TOURMALINE CRYSTALS

[From the *Scientific Proceedings of the Royal Dublin Society.* Read January 19, 1880.]

In the Philosophical Magazine for July, 1879, Professor Sylvanus Thompson and Dr. Oliver Lodge give the results of some very interesting experiments upon the unilateral conductivity of tourmaline crystals for heat and electricity. Dr. Lodge had shown that an explanation of pyro-electricity might be given if such crystals possessed a unilateral conductivity for electricity. A body is said to possess unilateral conductivity for anything if it conducts better in one direction than in the opposite one; as, for example, a tube with a series of funnels in it all turned the same way for fluids, and apparently in the case of Geissler's tubes for electricity also. The result of their experiments was that tourmaline crystals do possess a unilateral conductivity for heat *as long as their temperature is variable*, and similarly for electricity *as long as the temperature varies*. The first of these facts is an important and valuable increase of our knowledge, but the latter as they point out is, of course, only due to the already known electromotive force which constitutes their pyro-electric properties. They seem to have been dissatisfied with these results, for they had hoped to discover unilateral electric conductivity independently of changes of temperature. They do not seem to have noticed that what analogy should have led them to look for was unilateral conductivity *during changes of intensity of the current*. It is to be hoped that, as they possess a very fine specimen of tourmaline, they will continue their investigations

into this point. In the meanwhile it may be worth noticing a mechanical illustration of how this might be connected with pyro-electricity. Suppose a wire carrying a current, surrounded by a number of magnets, and that a majority of them pointed in one direction round the wire, and that each was kept in its place by a spring. On passing a current through the wire, all the magnets that did not point round it in a particular direction would tend to set themselves in this direction, and during changes of intensity of the current work would be done against or by the springs. If the current passed in such a direction that the majority of the magnets were set so as to remain unchanged, there would be less work done by changes of intensity than if the current were in the opposite direction, and this would give rise to an apparent unilateral conductivity. I say "apparent," because the weakening of the current is due to an inverse electromotive force, and not to a true increase of the resistance. The same effect would be produced by supposing a majority of the magnets turned in the same direction along the wire, and kept in possession by two springs, one on each side, but one stronger than the other, when of course a current would have to do more work in turning them to one side than to the other, so that in this case also there would be apparent unilateral conductivity during variations of the current.

Now, suppose that the proportion of polarized magnets or their strength depended on the temperature of the system. It is then evident that during changes of temperature there would be changes in the numbers or strengths of the polarized magnets; either would produce an electromotive force in the wire during the change. Hence the phenomena of pyro-electricity would be manifested by such a system. I put these forward merely as illustrations, not supposing that the structure of tourmaline is necessarily at all *like* either of them, but there are generally great analogies between different systems exhibiting the same phenomena, and an illustration gives us a concrete stepping-stone to found our conceptions on during the difficult transit to the abstract.

The passage of a current through an iron wire is accompanied by the production of a series of magnetic elements round it, and the effect of this has been noticed as causing an apparent change of resistance during changes of the current; but as there is no want of symmetry in the wire, there is no apparent unilateral conductivity.

15

NOTE ON THE CONSTRUCTION OF GUARD-RING ELECTROMETERS

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read January 19, 1880.]

GUARD-RING electrometers have usually been constructed with an aluminium disk, for the sake of lightness, surrounded by a guard-ring of brass. It is essential for the accuracy of the calculation of the absolute values of capacity and quantity made with them that the electricity should be as uniformly distributed as possible on the surface of the disk and guard. It is for the sake of producing a uniform distribution on the disk that the guard is added. Hence any arrangement which disturbs this uniformity of distribution is to be avoided. Now, whether the contact of dissimilar metals in itself produces an appreciable difference of potential between them, or whether it is the air near different metals that is at different potentials, there is no doubt that when the plates of an accumulator are of different metals there is an appreciable accumulation of electricity upon them. Consequently, in the guard-ring electrometer, the distribution of electricity on the aluminium disk cannot be the same as on the brass guard connected with it. It might seem as if the other plate should also be of the same material, but as it is generally easy to apply the differential method of measurement, a constant, even though unknown, difference of potential between the plates is of no consequence.

16

NOTES ON FLUORESCENCE

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read May 19, 1880.]

THE object of this note is to call attention to the fact that, if a fluorescent body be heated sufficiently, it must cease to be fluorescent.

The method by which I arrived at this conclusion was very cumbrous, and when the note was communicated to the Royal Dublin Society, Dr. Stoney pointed out that a great simplification might be effected by using the method of proof ordinarily employed to show that the emissive and absorptive powers of a body are equal for each separate ray. In fact, fluorescence consists in a body's absorbing rays of one refrangibility and emitting them at another, and consequently for these absorbed rays the emissive power of the body cannot be equal to its absorptive power. The proof, however, assumes that the bodies are at the same temperature, and does not apply to any cases of fluorescence actually observed.

Let A_r be the absorption of an ordinary unfluorescent body, and E_r its total emission, and R_r its reflection of rays that do not produce fluorescence in the other body; and let A_v, E_v, and R_v be its absorption, emission, and reflection of rays that do produce fluorescence. For the fluorescent body, let a_r, e_r, and r_r be the corresponding quantities for the non-fluorescent rays, and a_v, e_{rv}, e_v, and r_v for the fluorescent rays; e_{rv} represents the rays absorbed as fluorescent, but emitted as non-fluorescent.

Now, suppose the two bodies to be spread over two infinite

parallel planes with perfectly reflecting surfaces, so as to avoid the necessity of considering transmitted rays, then for the non-fluorescent body we must have

$$A_r = E_r - R_r, \quad \text{and} \quad A_v = E_v - R_v;$$

for the absorbed rays must be the difference between the total emission and the reflection, as otherwise the temperature would change; and, as there is no alteration of refrangibility in this case, it must be true of each ray separately.

In the case of the fluorescent body we must have

$$a_r = e_r - r_r, \quad a_v = e_v + e_{rv} - r_v;$$

i.e., the absorbed non-fluorescent rays must be the difference between the emitted and reflected ones, and the absorbed and reflected fluorescent rays must together make up the whole emitted as fluorescent or non-fluorescent.

We have, besides, that all the fluorescent rays emitted by the non-fluorescent body must be either absorbed or reflected by the other body, so that

$$E_v = a_v + r_v = e_v + e_{rv}.$$

Also, all the fluorescent rays emitted by the fluorescent body are either absorbed or reflected by the other, so that

$$e_v = A_v + R_v = E_v;$$

and we thus immediately obtain that $e_{rv} = 0$, or there can be no fluorescence between bodies at the same temperature.

It seems likely from this that fluorescence depends on the amplitude of the incident vibration being much greater than that of the corresponding vibration of the molecule of the body; and that whatever causes the actual distribution of amplitudes of vibrations of the different periods produces its effect less completely in the case of fluorescent bodies than of non-fluorescent, and less rapidly in the case of phosphorescent than non-phosphorescent bodies.

17

ON THE POSSIBILITY OF ORIGINATING WAVE DISTURBANCES IN THE ETHER BY MEANS OF ELECTRIC FORCES

[From the *Scientific Transactions of the Royal Dublin Society.* Read November 17, 1879.]

In Professor J. Clerk Maxwell's most admirable treatise on "Electricity and Magnetism" the following expression occurs (§ 785):—"Let us suppose that when t [time] is zero, the quantities $\mathfrak{A}$ and $\dot{\mathfrak{A}}$ are zero except within a certain space S," and he proceeds to show that this will give rise to an electrical disturbance propagated in free space with the velocity of light. Some doubt as to the possibility of producing a distribution of currents which would originate such disturbances was aroused in my mind by the following consideration: for the validity of the deduction it is necessary that the whole space considered be non-conducting. Gauss showed long ago that such a quantity as the potential of any system of attracting bodies cannot have a zero value throughout one part of space and another value in any other communicating part. The components of $\mathfrak{A}$, the vector potential of the electric current, are of the same form as the potentials of attracting bodies, and could not consequently fulfil the condition Professor Maxwell assumes. Though this does not include the case of currents distributed throughout space, such as the changes of electric displacement which Professor Maxwell supposes to be currents,* yet I believe

[* Gauss' theorem is limited to space free of currents, and so ceases to hold when the front of the wave is crossed; so that there is no discrepancy.

The two ways of looking at Maxwell's vector potential $\mathfrak{A}$, either as propagated from the seats of the *true* electric disturbances, or as an instantaneous mathematical

that the reasoning may be extended to this case also. Further, it has to be shown that no other possible assumed distribution would give rise to disturbances propagated in time throughout space.

If we investigate how such disturbances could be originated by combinations of currents and charges on fixed or movable conductors and non-conductors, it is in the first place to be observed that we may legitimately assume the conductors to be perfect conductors, for the heating of a conductor by an electric current, as far as it is a production of waves in the ether, is, according to Professor Maxwell's hypothesis, one of the very things whose origin we want to find out, and so far as it is only an increased motion of matter is not related to the question in hand.

Now, if any system of conductors, movable or fixed, could give rise to the production of disturbances propagated throughout space, in time the energy of the system would become gradually expended in the same manner as heat is when a hot body cools. Another effect of such a system would be, that at each point of space there would generally be a double action, one the direct inductive action which Professor Maxwell never assumes to be propagated in time, and the other the effect of the wave on arriving at the point.

Now, if we consider the case of any system of perfect conductors and non-conductors, movable or fixed, and charged in any way, and carrying any currents, the interactions of the various parts may be worked out, either upon the hypothesis of direct

potential of the *total* circuital electric current which includes ethereal displacement as well as electric, has always been a fruitful source of misconception. The matter is practically cleared up in the following papers on electric radiation: cf. Nos. 19, 20, 24; cf. also Introduction; but the question has nevertheless continually recurred, how, in a theory of pure propagation in time, an instantaneous potential apparently not propagated can arise. The answer is in fact that from that point of view the potential is in part that of ethereal displacement, which is not electric disturbance but merely a result thereof like the potential itself. The molecular theory foreshadowed on p. 92 has since been developed by Lorentz mainly on the lines of the first of the two methods of exposition above mentioned, and by Larmor mainly on those of the second method.]

action at a distance, or upon Professor Maxwell's hypothesis of action through a medium. In the former method there is, however, no account taken of the non-conductor, nor are any variables representing it in any way involved; and yet, if we are dealing with perfect conductors, such a system would be, as regards energy, perfectly conservative, and so cannot be such as would give rise to a disturbance propagated throughout space in time like light.

I conclude from this that the origination of such disturbances is not a phenomenon of electric currents such as we have to deal with, but is connected with the relations of matter and ether, and this is probably an atomic interaction, as spectroscopic phenomena also seem to show, and will be explained only when some workable hypothesis as to the nature of this interaction has been sufficiently investigated.

If direct action at a distance, and Professor Maxwell's hypothesis of action through a medium, lead to the same results, as has been, I believe rightly, just now assumed,* then we may assert a very general theorem concerning the displacement currents which Professor Maxwell assumes in the non-conductor, and one which I have not yet succeeded in proving directly—namely: that however these may be produced by any system of fixed or movable conductors charged in any way, and discharging themselves amongst one another, they never will be so distributed as to originate wave disturbances propagated through space outside the system.

[* The exception to this statement is just the domain of ethereal disturbance involving radiation, that is here discussed. For correction see the papers that follow: cf. p. 101.]

18

ON THE POSSIBILITY OF ORIGINATING WAVE DISTURBANCES IN THE ETHER BY MEANS OF ELECTRIC FORCES. PART 2.

[From the *Scientific Transactions of the Royal Dublin Society.* Read May 19, 1880.]

IN a former paper, read before the Royal Dublin Society in November, 1879, upon the present subject, I showed that if the theories of action at a distance and Clerk Maxwell's of action through a medium lead to the same results in all cases, then there can be no production of waves propagated like light by means of such electrostatic or electrodynamic systems. I did not then point out where Clerk Maxwell practically assumes that this is the case, but he evidently does so when he assumes that the electrokinetic energy of a system of conductors carrying currents is a function of the present currents and their configuration only.* By assuming this he has excluded the possibility of wave-production, for in the case of wave-production part of the energy of the system is in the medium, and what part is so depends on the past as well as on the present configurations and currents.

There seems a very great difficulty in determining what is the actual distribution of displacement currents in the neighbourhood of a changing current in a conductor. We cannot assume them to be simply due to the variations of current in the conductor, for such displacement currents immediately react on one

[* This *total* current of Maxwell's is however not all *true* electric current; see footnote, p. 90, also the Introduction.]

another. We cannot even assume that they are initially the same as if they were directly and simply produced by the current in the conductor, for then this initial state would be a discontinuous one. The distribution at the limits of space, as well as time, are just as difficult to determine.

It is, however, possible to assume a distribution of electromagnetic potential in the neighbourhood of a conductor such that its components satisfy Maxwell's equation*

$$\Delta^2 V + K\mu \frac{d^2 V}{dt^2} = 0,$$

and yet such that it shall not represent a wave-propagation such as light, but rather the state of vibration in an organ-pipe where there are fixed nodes.

If we assume that V can be expanded in sines or cosines with regard to the time so that we may write

$$V = \Sigma V_n \begin{matrix}\sin \\ \cos\end{matrix} (nt),$$

where V_n is a function of x, y, z without t, then if we determine V_n suitably for a simple periodic $V = V_n \cos nt$, we see that the periods of maximum and minimum occur simultaneously everywhere so that it need not be of the nature of a wave-propagation like light. Now if

$$r = \sqrt{(x-a)^2 + (y-\beta)^2 + (z-\gamma)^2}, \text{ then } V_n = \frac{\begin{matrix}\sin \\ \cos\end{matrix}(\sqrt{K\mu}nr)}{r}$$

satisfies the equation

$$\Delta^2 V_n = n^2 K\mu V_n,$$

and we may get a more general form for V_n by taking it

$$V_n = \iiint f(a, \beta, \gamma) \frac{\begin{matrix}\sin \\ \cos\end{matrix}(\sqrt{K\mu}nr)}{r} da d\beta d\gamma,$$

and we can make it have given limiting values by determining $f(a, \beta, \gamma)$ rightly. This value of V_n vanishes for $r = \infty$.

[* Δ^2 has here the negative sign, as in the quaternion usage.]

We thus see that as

$$\frac{d^2V}{dt^2} = -\Sigma V_n n^2 \begin{matrix}\sin\\ \cos\end{matrix}(nt),$$

and

$$\Delta^2 V = K\mu \Sigma V_n n^2 \begin{matrix}\sin\\ \cos\end{matrix}(nt),$$

we get that V satisfies the given equation

$$\Delta^2 V + K\mu \frac{d^2V}{dt^2} = 0.$$

We thus see, generally, that if V contain a part depending on the time, and a part independent of it, then it must be of the form just determined, in order to satisfy this equation.

We have then that V satisfies the given equation if it is expressible thus:

$$V = \Sigma \iiint f(\alpha,\beta,\gamma) \frac{\begin{matrix}\sin\\ \cos\end{matrix}(\sqrt{K\mu}nr)}{r} \begin{matrix}\sin\\ \cos\end{matrix}(nt)\, d\alpha d\beta d\gamma.$$

This, of course, *may* represent a wave-propagation,* but a simply periodic variation of V in one curve in space would produce simultaneous maxima and minima values throughout space, and so would not originate wave-disturbances at all.

There is another method by which we may arrive at this same distribution of displacement currents near a varying current.

Assume an initial distribution of currents (U) in conductors only, and calculate the electromagnetic potential at each point due to them, and then in terms of the initial acceleration of current ($\ddot{U}$) calculate the initial displacement current (u) throughout space. We may not assume this to be the total initial displacement current, for we must go on and calculate the acceleration ($\ddot{u}$) of this, and find the resultant displacement currents and so on. Then by summing up the resultant infinite number of these partial initial currents, we may assume that the resultant is the actual initial current at each point.

[*Standing waves as distinct from progressive ones: see p. 123 for the latter.]

As we may deal with each element of current separately, I shall confine myself to a current in the direction of x, and then the only component of the electromagnetic potential at each point is F, and

$$F_0 = \mu \frac{U}{R},$$

(I distinguish the points at which F exists by the suffix) where μ is the magnetic inductive capacity of the medium, and R is the distance of the point $_0$ from the current. The current at $_0$ produced by the acceleration of U is

$$u_0 = -\frac{K}{4\pi}\ddot{F} = -\frac{K\mu}{4\pi}\frac{\ddot{U}}{R}, \text{ writing } \ddot{U} \text{ for } \dot{U}.$$

Hence the vector potential at $_1$ due to this is

$$F_1 = -\frac{K\mu}{4\pi}\ddot{U}\int\frac{dm_0}{r_{10}R},$$

where dm_0 is the element of the volume at $_0$, and r_{10} is the distance from $_0$ to $_1$. Similarly

$$F_2 = \left(\frac{K\mu}{4\pi}\right)^2 \dddot{U}\int\frac{dm_1}{r_{21}}\int\frac{dm_0}{r_{10}R},$$

and generally

$$F_n = (-1)^n\left(\frac{K\mu}{4\pi}\right)^n \overset{2n}{U}\int\frac{dm_{n-1}}{r_{nn-1}}\cdots\int\frac{dm_0}{r_{10}R};$$

hence, if we call

$$\rho_n = \frac{1}{(4\pi)^n}\int\frac{dm_{n-1}}{r_{nn-1}}\cdots\int\frac{dm_0}{r_{10}R},$$

we may write

$$F_n = (-1)^n(K\mu)^n \overset{2n}{U}\rho_n,$$

and the relation connecting ρ_n and ρ_{n-1} is

$$4\pi\rho_n = \int\frac{\rho_{n-1}dm_{n-1}}{r_{nn-1}}.$$

Before going further with this I may call attention to this,

that if U were a simply periodic function of the time $= a \cos mt$, we can see that the complete F

$$F = \Sigma_0^\infty F_n,$$

would necessarily be of the form

$$\cos mt \; \Sigma(K\mu)^n m^{2n} \rho_n = V_m \cos mt,$$

so that if F is to satisfy the equation

$$\Delta^2 F + K\mu \ddot{F} = 0,$$

we can see that V_m must be as before

$$V_m = A_m \frac{\begin{matrix}\sin \\ \cos\end{matrix} (\sqrt{K\mu}\, mR)}{R},$$

for it can only be a function of R as is evident.

We may get this otherwise by proceeding to determine ρ_n from ρ_{n-1}, for we know that $\rho_0 = \frac{1}{R}$. We may do this with facility by observing that as ρ_{n-1} is a function of the coordinates of the point $_{n-1}$ only we have

$$\Delta^2 \rho_n = \rho_{n-1},$$

or as ρ_n and ρ_{n-1} are all functions of R only we have that this reduces to

$$\frac{d}{dR}\left(R^2 \frac{d\rho_n}{dR}\right) + R^2 \rho_{n-1} = 0,$$

or what is the same integrated

$$\rho_n = -\int \frac{dR}{R^2} \int R^2 \rho_{n-1} dR.$$

From this we obtain, neglecting the constants introduced by integration which do not affect the form of the result,

$$\rho = \frac{1}{R}, \quad \rho_1 = -\frac{R}{2}, \quad \rho_1 = \frac{R^3}{\lfloor 4} \cdots \rho_n = \frac{(-1)^n R^{2n}}{R \lfloor 2n},$$

so that we evidently have

$$F_n = \frac{(K\mu)^n}{R} \overset{2n}{U} \frac{R^{2n}}{\lfloor 2n}.$$

Now, in order to sum the results, we must, as before, split up U in its components by expanding it in a series of sines and cosines, when we may evidently take

$$F_n = \Sigma A_m \frac{\cos mt}{R} (-1)^n (K\mu)^n m^{2n} \frac{R^{2n}}{\lfloor 2n},$$

and $F = \Sigma F_n$; therefore

$$F = \Sigma A \cos mt \frac{\cos m\sqrt{K\mu}R}{R},$$

which is of the same form as before, as we saw it ought to come out.

There are some obvious objections to the latter part of this enumeration, in which several of the terms are infinite if the space be infinite.

The only assumption I seem here to have made is that the initial disturbance is the sum of all the initial induced currents, and this seems the only natural and an almost necessary assumption.

As $\frac{1}{\sqrt{K\mu}}$ is the velocity of light, $\sqrt{K\mu}$ is very small, so that, with currents varying at the rate they usually do, $\cos m\sqrt{K\mu}R = 1$ for any not very large value of R and F, and so has the same value as if there were no induced displacement currents at all.

19

ON THE POSSIBILITY OF ORIGINATING WAVE DISTURBANCES IN THE ETHER BY MEANS OF ELECTRIC FORCES—CORRECTIONS AND ADDITIONS

[From the *Scientific Transactions of the Royal Dublin Society.* Read May 5, 1882.]

SINCE publishing Part II of these papers (Trans. R.D.S., 1882, p. 173), I have come across an investigation of the differential equation I there studied in Lord Rayleigh's "Theory of Sound," Vol. II, § 276. He assumes the solution to contain imaginary terms of the form c^{int}, just as I assumed it to contain real terms of the form $\cos nt$, and he obtains the same general form of solution as I do. To a simply periodic term, however, there corresponds in his solution a term of the form

$$A \frac{\cos (nt - mr)}{r},$$

instead of the term I obtained,

$$A \frac{\cos nt \cos mr}{r}.$$

Taking Lord Rayleigh's form of solution would lead to the conclusion that a simply periodic current would originate wave disturbances such as light, and not the stationary waves that my solution leads to.

I have not been able, from purely mathematical considerations, to determine which form of solution is preferable. Each is only a partial and not a general solution, and the form depends on the limiting conditions which are the same in both, when r

is small and when it is infinite.* When mr is small, the two forms are indistinguishable, and it is not easy to make experiments in which mr is not small, and I know of none that can help to decide between the forms. If we could make experiments where $mr = \frac{1}{2}\pi$, we could easily distinguish between them; but as this would be at a distance of several miles from the varying current when the variations were as rapid as in the highest audible note, there does not seem much hope from experiments in this direction. Dr. Oliver Lodge has already tried to originate light by induced currents of a high order. It does not seem that this method of producing very rapidly varying currents would succeed beyond the third or fourth orders, for the induced currents of the higher orders tend more and more to become simply periodic, and after that there is only one induced current corresponding to each inducing one. It might, however, be possible to obtain sufficiently rapidly alternating currents by discharging condensers through circuits of small resistance.† The only experiments with which I am acquainted on the magnetic effects of such discharges seem to indicate a periodic law for the induction near the circuit.

In the theory of sound it is known that the disturbances are propagated outwards, and that the energy of the vibrating body is gradually transferred to the medium, and so we know which solution to employ. If a body be sounding in the centre of a closed sphere of the right size during the time that the sound takes to reach the surface of the sphere, the first form of solution holds; but as soon as the reflected wave combines with the direct one, the second form is the right one. If the electromagnetic action is analogous to this, it seems to follow that, in infinite space, where there is nothing to produce the reflected wave, the first form of solution, in which all the energy is gradually transferred to the medium, is the right one to employ.

[* This is answered in the next paragraph; the standing waves arise from the superposition of a train travelling outwards and another travelling inwards, and the latter can only become established by aid of a reflecting outer boundary.]

[† This is of course the method by which Tesla afterwards developed his striking luminous effects of induction at high tension.]

Indeed it seems almost impossible without calling in the aid of some form of direct action at a distance to see how the stationary waves can be simultaneously originated throughout space.

If, however, as is generally assumed, a system of perfect conductors carrying currents be a conservative system, they cannot be analogous to a system such as a sounding body in air; for if they were, and the currents vary in intensity, they would gradually transfer their energy to the ether. In my first paper on this subject I investigated it from this point of view. Assuming that the theory of direct action at a distance, and Maxwell's of action through a medium, lead to the same results, I showed that electric currents could not originate wave disturbances such as light. From my subsequent paper, it is, however, clear that the assumption that the two theories lead to the same results is only true to the same order of approximation as omitting the mutual induction of the displacement currents in the non-conducting medium. This is evident from the fact that the equations for calculating the vector potential on the theory of direct action at a distance could never make it vanish short of infinity, while the equations founded on Maxwell's theory certainly lead to the conclusion that either the energy is gradually transferred to the medium or else that the vector potential vanishes at each of a series of distances given by an equation of the form $\cos mr = 0$. Hence these papers at least show that, contrary to the ordinary assumption, the two theories do not lead to the same results, and they point out a direction in which to investigate which theory is true. It seems further highly probable that the energy of varying currents is in part radiated into space and so lost to us. It seems further probable that, contrary to what I have stated in my first paper, the interactions between the molecules of matter and the ether are of the same character as the electromagnetic actions with which we are acquainted.

In conclusion, I must apologize for having ventured to investigate these matters when I was so ignorant of what had already been done as to make mistakes requiring such serious corrections as are contained in this paper.

20

NOTE ON MR. J. J. THOMSON'S INVESTIGATION OF THE ELECTROMAGNETIC ACTION OF A MOVING ELECTRIFIED SPHERE

[From the *Scientific Proceedings* (Vol. III, Part V) *of the Royal Dublin Society*. Read November 21, 1881.]

IN the April number of the Philosophical Magazine for the present year Mr. J. J. Thomson has given an interesting investigation of the electromagnetic action of a moving electrified sphere.

On the fourth page (*l. c.* p. 232) of this investigation, he makes an assumption which he does not justify in order to make the components of the vector potential of electromagnetic induction satisfy the condition

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

As it seemed very likely that they ought to satisfy this condition, I thought it worth while bringing before the Society a justification of his assumption which, however, leads to slightly different equations from his, though his final result is unaffected.

Mr. Thomson has not touched the question of the discontinuity at the surface of the sphere, nor what becomes of the displacement when the sphere passes over a point. We may assume that the point remains in its displaced position, and this is practically what Mr. Thomson assumes; but if we do, the above condition is not fulfilled. We may assume that it returns to its original position, so that no permanent displacement takes place in the track of the sphere, as would occur on Mr. Thomson's assumption. This, however, does not satisfy the condition

either, and I have been led to assume that the particle does return to its original position, but that in some way or other the discontinuity at the surface acts as if a moving quantity of electricity acted like an element of an electric current. This may seem like begging the question, but it is only doing explicitly what Mr. Thomson does implicitly. It is evidently impossible that the electromagnetic action of moving electricity can be due entirely to the electromagnetic action of the displacement currents in the dielectric, for in the case of a plane moving parallel to itself there are none of these displacement currents, and yet that is the only case that has been experimentally verified.

To show that my assumption leads to equations satisfying the condition, and leading to practically the same results as Mr. Thomson's, does not require much work.

Consider an elementary volume $dx\,ds\cos\theta$, where ds is an element of the surface of the sphere, and θ the angle the radius makes with x. The displacement in this volume is $= \delta$ the superficial density, while after the time dt it is zero on my assumption. Hence, calling the displacement D, we have

$$\frac{d\mathrm{D}}{dt}\,dt = -\,\delta dx ds\cos\theta.$$

Hence
$$\dot{\mathrm{D}} = -\,\delta\frac{dx}{dt}\,ds\cos\theta.$$

Now $\frac{dx}{dt} = p$ the velocity of the sphere which is supposed to be moving along x. Hence the components of $\dot{\mathrm{D}}$ are $\dot{f}$, $\dot{g}$, $\dot{h}$, and observing that $ds = a^2 d\mu d\phi$, where $\cos\theta = \mu$, and a is the radius of the sphere, while $4\pi a^2\delta = e$, the total quantity of electricity on the sphere,

$$\dot{f}\,dxdydz = -\frac{ep}{4\pi}\mu^2 d\mu d\phi,$$

$$\dot{g}\,dxdydz = -\frac{ep}{4\pi}\mu\cos\phi\,d\mu d\phi,$$

$$\dot{h}\,dxdydz = -\frac{ep}{4\pi}\mu\sin\phi\,d\mu d\phi.$$

Hence the components of the electromagnetic potential are at a point at a distance

$$F_e = -\frac{ep}{4\pi}\iint\frac{\mu^2}{r}\,d\mu d\phi,$$

$$G_e = -\frac{ep}{4\pi}\iint\frac{\mu\cos\phi}{r}\,d\mu d\phi,$$

$$H_e = -\frac{ep}{4\pi}\iint\frac{\mu\sin\phi}{r}\,d\mu d\phi.$$

These are the components of the electromagnetic potential due to this superficial change of displacement that I have assumed. When integrated over the surface of the sphere they give at a point distant R from its centre, and whose polar angles are α and ε,

$$F_e = -\frac{ep}{3R} - \frac{epa^2}{5R^3}(\cos^2\alpha - \tfrac{1}{3}).$$

$$G_e = -\frac{epa^2}{5R^3}\cos\alpha\sin\alpha\cos\varepsilon,$$

$$H_e = -\frac{epa^2}{5R^3}\cos\alpha\sin\alpha\sin\varepsilon.$$

If we add to these the components calculated by Mr. Thomson, as due to the external displacement currents, and given by him (*l. c.* p. 233), namely,

$$F_e = \frac{ep}{10R^3}(5R^2 - 3a^2)(\cos^2\alpha - \tfrac{1}{3}),$$

$$G_e = \frac{ep}{10R^3}(5R^2 - 3a^2)\cos\alpha\sin\alpha\cos\varepsilon,$$

$$H_e = \frac{ep}{10R^3}(5R^2 - 3a^2)\cos\alpha\sin\alpha\cos\varepsilon,$$

we get as the resultant values of the components produced by the displacements assumed,

$$F_d = -\frac{ep}{3R} + \frac{ep}{2R^3}(R^2 - a^2)(\cos^2\alpha - \tfrac{1}{3}),$$

$$G_d = \frac{ep}{2R^3}(R^2 - a^2)\cos\alpha\sin\alpha\cos\varepsilon,$$

$$H_d = \frac{ep}{2R^3}(R^2 - a^2)\cos\alpha\sin\alpha\sin\varepsilon,$$

which however do not satisfy the condition

$$\frac{dF_d}{dx} + \frac{dG_d}{dy} + \frac{dH_d}{dz} = 0.$$

Now it is very easy to calculate the action of the superficial moving electricity if it be assumed to act like an electric current. Each element of the surface will act as if it had an electric current-element δp on it, and the x components of the electromagnetic potential will evidently be the same as those of an electrostatic potential, while the y and z components will vanish.

Hence $$F_e = \frac{\mu e p}{R}, \quad G_e = H_e = 0.$$

My assumption is that the complete components are

$$F = F_e + F_d, \quad G = G_e + G_d, \quad H = H_e + H_d;$$

and it is easy to see that they can be put in the form

$$F = \frac{\mu e p}{R} + \frac{\mu e p}{6}\left\{(R^2 - a^2)\frac{d^2}{dx^2}\frac{1}{R} - \frac{2}{R}\right\},$$

$$G = \frac{\mu e p}{6}(R^2 - a^2)\frac{d^2}{dxdy}\frac{1}{R},$$

$$H = \frac{\mu e p}{6}(R^2 - a^2)\frac{d^2}{dxdz}\frac{1}{R},$$

and these satisfy the condition

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

These may be further simplified by assuming a function

$$\chi = -\frac{\mu e p}{2}\left(x + \tfrac{1}{3}a^2\frac{d}{dx}\right)\frac{1}{R};$$

thus $$F = \frac{\mu e p}{R} + \frac{d\chi}{dx},$$

$$G = \frac{d\chi}{dy},$$

$$H = \frac{d\chi}{dz}.$$

We hence easily see that, in the general case where the sphere is moving with component velocities p, q, r, we must take

$$\chi = -\frac{\mu e}{2}\left\{px + qy + rz + \tfrac{1}{3}a^2\left(p\frac{d}{dx} + q\frac{d}{dy} + r\frac{d}{dz}\right)\right\}\frac{1}{\mathrm{R}},$$

or calling

$$p\frac{d}{dx} + q\frac{d}{dy} + r\frac{d}{dz} = \frac{d}{d\theta},$$

and observing that

$$p\frac{x}{\mathrm{R}} + q\frac{y}{\mathrm{R}} + r\frac{z}{\mathrm{R}} = \dot{\mathrm{R}},$$

we have

$$\chi = -\frac{\mu e}{2}\left(\dot{\mathrm{R}} + \tfrac{1}{3}a^2\frac{d}{d\theta}\frac{1}{\mathrm{R}}\right);$$

and then we have

$$\mathrm{F} = \frac{\mu e p}{\mathrm{R}} + \frac{d\chi}{dx},$$

$$\mathrm{G} = \frac{\mu e q}{\mathrm{R}} + \frac{d\chi}{dy},$$

$$\mathrm{H} = \frac{\mu e r}{\mathrm{R}} + \frac{d\chi}{dz}.$$

From these it appears at once that the magnetic effect of the displacement currents is nil. For the components of the magnetic force α, β, γ are

$$\alpha = \frac{d\mathrm{G}}{dz} - \frac{d\mathrm{H}}{dy},$$

$$\beta = \frac{d\mathrm{H}}{dx} - \frac{d\mathrm{F}}{dz},$$

$$\gamma = \frac{d\mathrm{F}}{dy} - \frac{d\mathrm{G}}{dx},$$

and χ disappears from this, and there only remains the magnetic action due to the current that I have assumed to represent the moving superficial electricity. The effect of this is obviously the same as if the whole quantity of electricity were moving at its centre, and this is the same as Mr. Thomson's result (*l. c.* p. 236).

It is to be observed that Mr. Thomson has intentionally omitted the self-induction of these displacement currents on one another, and it may legitimately be omitted when the motion is comparatively slow, but a complete solution of the question would be most interesting.*

It may be worth while remarking that no effect except light has ever yet been traced to the displacement currents assumed by Maxwell, in order to be able to assume all currents to flow in closed circuits. It has not, as far as I am aware, been ever actually demonstrated that open circuits, such as Leyden jar discharges, produce exactly the same effects as closed circuits, and until some such effect of displacement currents is observed, the whole theory of them will be open to question.

[* Given by Heaviside, *Phil. Mag.*, 1889, or "Electrical Papers," ii, p. 504.]

21

ON COMETS' TAILS

[From the *Scientific Proceedings of the Royal Dublin Society* (Part VI, Vol. III). Read May 5, 1882.]

IN the last edition of Clerk Maxwell's "Electricity and Magnetism," p. 402, § 793, he calculates that, according to the electromagnetic theory of light, sunlight should exert on a body that entirely absorbed it, a pressure equivalent to

$$P = 4{\cdot}22 \times 10^{-5} \text{ dynes per sq. centim.}$$

Now at the distance of the Earth the attraction of the Sun for a mass m is

$$f = {\cdot}59 \times m \text{ dynes.}$$

These forces might balance if the absorbing surface were sufficiently large in proportion to the mass, so that if s were the surface

$$s \times 4{\cdot}22 \times 10^{-5} = {\cdot}59 \times m,$$

or

$$m = s \times 7 \times 10^{-5}.$$

Now the mass of a molecule of a gas of density Δ at the ordinary pressure and temperature is

$$m = \Delta \times 10^{-21},$$

so that

$$s = 10^{-17} \times 1{\cdot}4 \times \Delta.$$

This gives the surface of a molecule of a gas whose density is Δ in order that it should be neither attracted nor repelled by the Sun. If this were the case at one distance, it would be so at all distances, because both the attraction of the Sun and the

intensity of the light vary inversely as the square of the distance To find whether this is possible we must make some assumption as to its shape, and I will assume that the molecule is spherical, which will have as small a surface as possible, and apply it to the case of hydrogen. In this case assume ν the radius of a molecule. Then evidently

$$s = \pi\nu^2 = 3{\cdot}1 \times \nu^2 = 1{\cdot}4 \times 10^{-17} \times \Delta;$$

$$\therefore \quad \nu^2 = 4{\cdot}4 \times 10^{-18} \times \Delta.$$

In the case of hydrogen

$$\Delta = 8{\cdot}8 \times 10^{-5};$$

$$\therefore \quad \nu^2 = 38{\cdot}72 \times 10^{-23};$$

$$\therefore \quad \nu = 2 \times 10^{-11} \text{ nearly,}$$

so that this is still only one-tenthousandth part of the mean distance between them, and is considerably less than the size of a molecule as usually estimated. Of course no gas would absorb all the light that falls on it; but from these calculations it seems possible that a light gas, with large molecules, such as some of the complex hydrocarbons, that absorbed a considerable proportion of the radiations that fall on them, might not only be in equilibrium under the attraction of the Sun and the repulsion of its radiations, but might be repelled by the Sun with considerable force. It has been lately shown that a repulsive force emanating from the Sun would account for comets' tails. The force, however, must not be like gravitation, proportional to the mass of of the body, for some comets have more than one tail, and this requires a different acceleration on some kinds of the cometary matter from that on others. This would be completely accounted for by the hypothesis I have put forward. The repulsion of the Sun depends on the surface of the molecule and on its absorbing power for heat, and these are not by any means proportional to its mass.

NOTE ADDED IN PRESS.—August 1, 1882.

Another method can be founded on calculating the proportion of heat that should be absorbed by the unit volume of a substance in order that it be neither attracted nor repelled by the Sun. As before the pressure on a completely absorbing unit volume would be

$$P = 4{\cdot}2 \times 10^{-5},$$

so that, if a were the absorbing power, the force on it would be

$$F = 4{\cdot}2 \times 10^{-5} \times a.$$

On the other hand the attractive force on a unit volume of density Δ would be

$$F = {\cdot}59 \times \Delta.$$

Hence, in order that these might be equal, we must have

$$a = 1{\cdot}1 \times 10^{4} \times \Delta.$$

Now in the case of hydrogen at the usual temperature and pressure

$$\Delta = 8{\cdot}8 \times 10^{-5},$$

so that

$$a = {\cdot}96,$$

or it would have to absorb 96 per cent. of the radiations that fell on it. From this it seems almost certain that this repulsion can have hardly any effect on the motion of gases under the action of the Sun. As, however, but little is known of the thickness of the layer of gas that will absorb all the heat that the gas will easily absorb, it may be premature to decide finally upon the question.*

[* Reflection produces pressure, as well as absorption; and on the other side of the argument may be set the enormous total reflection of solar light by a comet's tail whose mass is entirely inappreciable. On the subject of repulsion by radiation, see Larmor, "Æther and Matter," 1900, chapter viii, on the theoretical side, and Lebedew, *Paris Congress Physical Reports*, 1900, on the experimental side.]

22

ON ELECTROMAGNETIC EFFECTS DUE TO THE MOTION OF THE EARTH.

[From the *Transactions of the Royal Dublin Society* (N. S., Vol. I). Read May 5, 1882.]

PROFESSOR ROWLAND has shown experimentally that a quantity of electricity moving acts like an electric current. This had been assumed by many physicists. It follows that two quantities of electricity moving in the same direction and with the velocity of light, would have no action on one another, their electrostatic action being balanced by an equal and opposite electrokinetic action. As it is very unlikely that anything depends on absolute motion, the motion here spoken of must be with respect to something, and this something can hardly be any other thing than what is known as the ether in space.

All electromagnetic measurements are made on the surface of the Earth, which is moving in a threefold way through space. The Earth is rotating on its axis, and its equatorial surface is moving at the rate of $4{\cdot}65 \times 10^4$ c.m. per sec. It is moving round the Sun at the rate of $2{\cdot}96 \times 10^6$ c.m. per sec. It is moving with the whole solar system through space, at about the same rate.

Professor Stokes has shown that aberration would be the same, whether the ether near the Earth be carried along with it or no. Fizeau's experiments, however, show that matter does not carry the ether along with it except to an extent depending on the refractive index of the matter. It would follow that

no appreciable part of the ether in the air moves with the Earth.

All electrostatic measurements should show a twofold periodicity; one diurnal, the other annual. The velocity of the electricity through the ether is different at different times of the day, owing to its motion due to the rotation of the Earth being sometimes added, and sometimes subtracted, from its other motions. It is different at different times of the year, owing to the velocity of the Earth round the Sun being sometimes added and sometimes subtracted from the component along the ecliptic of the solar system's velocity in space. The velocity of light is 3×10^{10} c.m. per sec., and the daily variation of velocity would consequently change electrostatic attractions by the $3{\cdot}1 \times 10^{-8}$ of their amount, and the annual variation would change them by about the $1{\cdot}5 \times 10^{-4}$ of their amount. These quantities do not seem beyond the possibility of observation with specially constructed instruments.

It has been supposed that in conformity with these electrostatic actions, there would be a force on a magnet, due to an electrified body moving along with the magnet. It appears to me that this is not the case. For, consider the action of a current on a quantity of electricity, when both are carried through space. When a current producing an electromagnetic potential, whose components are F, G, H, moves with a velocity of translation whose components are $\dot{x}$, $\dot{y}$, $\dot{z}$, the components of electromotive intensity are

$$P_1 = -\frac{dF}{dt} = -\left(\frac{dF}{dx}\dot{x} + \frac{dF}{dy}\dot{y} + \frac{dF}{dz}\dot{z}\right),$$

$$Q_1 = -\frac{dG}{dt} = -\left(\frac{dG}{dx}\dot{x} + \frac{dG}{dy}\dot{y} + \frac{dG}{dz}\dot{z}\right),$$

$$R_1 = -\frac{dH}{dt} = -\left(\frac{dH}{dx}\dot{x} + \frac{dH}{dy}\dot{y} + \frac{dH}{dz}\dot{z}\right).$$

If a point move with a velocity whose components are ξ, η, ζ, then the components of electromotive intensity at it are

$P_2 = c\eta - b\zeta$, &c., where a, b, c are the components of magnetic induction at the point. Putting in for a, b, c, we get

$$\begin{aligned} P_2 &= \eta\left(\frac{dG}{d\xi} - \frac{dF}{d\eta}\right) - \zeta\left(\frac{dF}{d\zeta} - \frac{dH}{d\xi}\right) \\ &= \dot{\xi}\frac{dF}{d\xi} + \dot{\eta}\frac{dG}{d\xi} + \dot{\zeta}\frac{dH}{d\xi} - \left(\dot{\xi}\frac{dF}{d\xi} + \dot{\eta}\frac{dF}{d\eta} + \dot{\zeta}\frac{dF}{d\zeta}\right) \\ &= \frac{d}{d\xi}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \dot{\xi}\frac{dF}{dx} + \dot{\eta}\frac{dF}{dy} + \dot{\zeta}\frac{dF}{dz}, \\ Q_2 &= \frac{d}{d\eta}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \dot{\xi}\frac{dG}{dx} + \dot{\eta}\frac{dG}{dy} + \dot{\zeta}\frac{dG}{dz} \\ R_2 &= \frac{d}{d\zeta}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \dot{\xi}\frac{dH}{dx} + \dot{\eta}\frac{dH}{dy} + \dot{\zeta}\frac{dH}{dz}. \end{aligned}$$

Hence the whole electromotive intensity at the point has components

$$\begin{aligned} P &= \frac{d}{d\xi}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \frac{dF}{dx}(\dot{\xi} - \dot{x}) + \frac{dF}{dy}(\dot{\eta} - \dot{y}) + \frac{dF}{dz}(\dot{\zeta} - \dot{z}), \\ Q &= \frac{d}{d\eta}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \frac{dG}{dx}(\dot{\xi} - \dot{x}) + \frac{dG}{dy}(\dot{\eta} - \dot{y}) + \frac{dG}{dz}(\dot{\zeta} - \dot{z}), \\ R &= \frac{d}{d\zeta}(F\dot{\xi} + G\dot{\eta} + H\dot{\zeta}) + \frac{dH}{dx}(\dot{\xi} - \dot{x}) + \frac{dH}{dy}(\dot{\eta} - \dot{y}) + \frac{dH}{dz}(\dot{\zeta} - \dot{z}). \end{aligned}$$

If the two move with the same velocity so that $\dot{\xi} = \dot{x}$, $\dot{\eta} = \dot{y}$, and $\dot{\zeta} = \dot{z}$, the components become

$$P = \frac{d}{d\xi}(F\dot{x} + G\dot{y} + H\dot{z}),$$

$$Q = \frac{d}{d\eta}(F\dot{x} + G\dot{y} + H\dot{z}),$$

$$R = \frac{d}{d\zeta}(F\dot{x} + G\dot{y} + H\dot{z}).$$

Now as

$$F = \mu\iiint\frac{u}{r}\,dxdydz,$$

$$G = \mu\iiint\frac{v}{r}\,dxdydz,$$

$$H = \mu\iiint\frac{w}{r}\,dxdydz,$$

where u, v, w are the components of the current, and

$$r^2 = (\xi - x)^2 + (\eta - y)^2 + (\zeta - z)^2,$$

if we call $$u\dot{x} + v\dot{y} + w\dot{z} = \Theta,$$

and $$\iiint \frac{\mu\Theta}{r}\, dx\, dy\, dz = \Psi,$$

we get

$$P = \frac{d\Psi}{d\xi},$$

$$Q = \frac{d\Psi}{d\eta},$$

$$R = \frac{d\Psi}{d\zeta}.$$

This result may be more rapidly obtained by using the quaternion notation thus:—

The electromotive intensity due to the current moving is

$$\mathfrak{E}_1 = -\frac{d\mathfrak{A}}{dt} = -\left(\dot{x}\frac{d\mathfrak{A}}{dx} + \dot{y}\frac{d\mathfrak{A}}{dy} + \dot{z}\frac{d\mathfrak{A}}{dz}\right) = (\mathrm{S}\dot{\rho}\Delta)\mathfrak{A},$$

using $$-\mathrm{S}\dot{\rho}\Delta = \dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}$$ as an operator.

Similarly the electromotive intensity at the moving point is

$$\mathfrak{E}_2 = \mathrm{V}\rho'\mathfrak{B}, \quad \text{and} \quad \mathfrak{B} = \mathrm{V}\Delta'\mathfrak{A}$$

where

$$\rho' = \xi i + \eta j + \zeta k \quad \text{and} \quad \Delta' = i\frac{d}{d\xi} + j\frac{d}{d\eta} + k\frac{d}{d\zeta}.$$

Hence $$\mathfrak{E}_2 = \mathrm{V}\dot{\rho}'(\mathrm{V}\Delta'\mathfrak{A}) = -\Delta'(\mathrm{S}\dot{\rho}'\mathfrak{A}) + (\mathrm{S}\dot{\rho}'\Delta')\mathfrak{A}:$$

also $$\Delta' = -\Delta:$$

hence $$\mathfrak{E} = \mathfrak{E}_1 + \mathfrak{E}_2 = -\Delta'(\mathrm{S}\dot{\rho}'\mathfrak{A}) - (\mathrm{S}(\dot{\rho}' - \dot{\rho})\Delta)\mathfrak{A},$$

so that if $\dot{\rho}' = \dot{\rho}$

$$\mathfrak{E} = -\Delta'(\mathrm{S}\rho\mathfrak{A}) = \Delta'(\Psi),$$

where $$\Psi = -\mathrm{S}\dot{\rho}\mathfrak{A} = \iiint \mu \frac{-\mathrm{S}(\dot{\rho}\mathfrak{C})}{\mathrm{T}r}\, dx\, dy\, dz,$$

and consequently $$\Theta = -\mathrm{S}(\dot{\rho}\mathfrak{C}).$$

Expressed thus, it is evident that the resulting electromotive force round a closed circuit would vanish, and that the electromotive intensity at each point of space is the same as that due to electricity of density $\mu\Theta$ at each element of current. The total quantity of electricity required for this is zero, in the case of a closed circuit, for

$$\iiint (S\dot{\rho}\mathfrak{C})\, dx\, dy\, dz = \iiint u\, dx\, dy\, dz = \iiint v\, dx\, dy\, dz = \iiint w\, dx dy dz = 0,$$

when the integration is extended round closed circuits. From this it is evident that an electric current in a conductor would have no electromotive intensity on a point outside it, and moving with its own velocity; for just as electricity enclosed in the conductor would have no effect outside, similarly the current in the conductor would induce on its surface exactly such a charge as would neutralise the action due to the motion of the current on all points moving with the same velocity. Hence the complete value for the electromotive intensity at a point, due to a current in a conductor, when both the point and conductor are moving is

$$\mathfrak{E} = (S(\dot{\rho}' - \dot{\rho})\Delta)\mathfrak{A},$$

i.e. its components are

$$P = \quad - \dot{x}) \frac{dF}{dx} + (\dot{\eta} - \dot{y}) \frac{dF}{dy} + (\dot{\zeta} - \dot{z}) \frac{dF}{dz},$$

$$Q = (\dot{\xi} - \dot{x}) \frac{dG}{dx} + (\dot{\eta} - \dot{y}) \frac{dG}{dy} + (\dot{\zeta} - \dot{z}) \frac{dG}{dz},$$

$$R = (\dot{\xi} - \dot{x}) \frac{dH}{dx} + (\dot{\eta} - \dot{y}) \frac{dH}{dy} + (\dot{\zeta} - \dot{z}) \frac{dH}{dz}.$$

The mechanical force at the point, as given by Maxwell, is

$$\mathfrak{F} = V\mathfrak{B}\mathfrak{C} - e\Delta\Psi,$$

i.e. its components are

$$X = cv - bw - e \frac{d\Psi}{dx},$$

$$Y = aw - cu - e \frac{d\Psi}{dy},$$

$$Z = bu - av - e \frac{d\Psi}{dz}.$$

The electromotive intensity at the point, as given by Maxwell, is

$$\mathfrak{E} = V\mathfrak{B}\dot{\rho} - \frac{d\mathfrak{A}}{dt} - \Delta\Psi,$$

i.e. it components are

$$P = c\dot{y} - b\dot{z} - \frac{dF}{dt} - \frac{d\Psi}{dx},$$

$$Q = a\dot{z} - c\dot{x} - \frac{dG}{dt} - \frac{d\Psi}{dy},$$

$$R = b\dot{x} - a\dot{y} - \frac{dH}{dt} - \frac{d\Psi}{dz}.$$

Now there does not seem any reason why the mechanical force should not be written

$$\mathfrak{F} = V\mathfrak{B}\mathfrak{C} + e\mathfrak{E},$$

i.e. its components are

$$X = cv - bw + eP,$$

$$Y = aw - cu + eQ,$$

$$Z = vu - av + eR,$$

for is it not the meaning of the electromotive intensity, that the mechanical force due to it on a quantity of electricity, e, is $e\mathfrak{E}$? Although in his enumeration of the equations of the electromagnetic field, Maxwell gives the equations as at first cited, yet in § 631, where he is deducing the general expression for the electrostatic energy of the field, he practically assumes that the second expression I have given is the true one. Hence the mechanical force at a point moving with a velocity $\dot{\rho}$ and carrying a current $\mathfrak{C}$, and charged with a quantity of electricity e, is

$$\mathfrak{F} = V\mathfrak{B}\mathfrak{C} + eV\mathfrak{B}\dot{\rho} - e\frac{d\mathfrak{A}}{dt} - e\Delta\Psi,$$

i. e. its components are

$$X = cv - bw + e(c\dot{y} - b\dot{z}) - e\frac{dF}{dt} - e\frac{d\Psi}{dx},$$

$$Y = aw - cu + e(a\dot{z} - c\dot{x}) - e\frac{dG}{dt} - e\frac{d\Psi}{dy},$$

$$Z = bu - av + e(b\dot{x} - a\dot{y}) - e\frac{dH}{dt} - e\frac{d\Psi}{dz}.$$

Now, when there is no conduction current at the point, $u = v = w = 0$. For it would evidently be improper to include e twice over, first as a quantity of electricity, and secondly as a current. In the equations as I give them, the effect of the motion of e is included under $c\mathrm{V}\mathfrak{B}\dot{\rho}$, *i.e.*, in the components, as $e(c\dot{y} - b\dot{z})$, &c. Hence we get that, if there be no conduction current at the point, but only this convection current, the mechanical force is completely represented by

$$\mathfrak{F} = e\mathfrak{E},$$

i.e., its components are

$$\mathrm{X} = e\mathrm{P},$$

$$\mathrm{Y} = e\mathrm{Q},$$

$$\mathrm{Z} = e\mathrm{R},$$

and that there will consequently be no mechanical force at points where the electromotive intensity vanishes.

It has just been shown that the electromotive intensity vanishes at a point outside a magnet, moving with the same velocity as the magnet, and I hence conclude that there would be no force on an electrified body moving with the magnet.

If we consider the electrified body as acting on the magnet we are led to a similar result. In this case the moving electricity has an electromotive intensity

$$\mathfrak{E} = \mu \iiiint\!\!\iiint \frac{e\dot{\rho}}{\mathrm{T}r}\, dx\,dy\,dz\,d\xi\,d\eta\,d\zeta,$$

or, as

$$\Psi = \iiint \frac{e}{\mathrm{T}r}\, dx\,dy\,dz,$$

$$\mathfrak{E} = \mu \iiint \Psi\dot{\rho}\, d\xi\,d\eta\,d\zeta,$$

i.e., its components are

$$\mathrm{P} = \mu \iiint \Psi\dot{x}\, d\xi\,d\eta\,d\zeta,$$

$$\mathrm{Q} = \mu \iiint \Psi\dot{y}\, d\xi\,d\eta\,d\zeta,$$

$$\mathrm{R} = \mu \iiint \Psi\dot{z}\, d\xi\,d\eta\,d\zeta.$$

Hence if Ψ be constant all over any space, the magnetic force vanishes throughout that space. Now Ψ must be constant all

over the inside of a magnet. Hence we see that the electricity induced on the outside of a magnet, by external electricity moving with it, exactly neutralises both the electrostatic and electromagnetic actions of the external electricity on the inside of the magnet. We thus see that the magnetism inside the magnet, and the convection current outside, are related to one another exactly like a quantity of electricity inside and outside a closed conductor; each induces on the conductor exactly such a charge as to neutralise its action on the other. The outstanding action on the conductor is exactly the same as if there were no internal charge at all. Hence, it seems that the action on a magnet would be independent of its magnetization, and would be only that feeble force due to the electromagnetic action of the superficial induced charge. It is useless then to expect to obtain measurable forces by the action of electricity on magnets, when both are carried along by the Earth.

It seems hardly necessary to point out that the action of two conduction currents upon one another is unaffected by their common motion, as equal quantities of positive and negative electricities are present at each point, and there is consequently no convection action.*

I have throughout this investigation left out of account, as too small to be worth including, the self-induction of the displacement currents produced in the dielectrics. It is, however, very desirable that their action should be completely investigated, in order to see whether they can produce measurable effects, for instance in the case of powerful electromagnets.†

* It may be worth while remarking that the actions of convection currents, which, with Maxwell's equations, seemed to need an additional assumption, come out as obvious results of the equations as here given.

[† See Lorentz, "Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern," Leiden, 1895; Larmor, "Æther and Matter," 1900, section iii.]

23

ON DR. EDDY'S HYPOTHESIS THAT RADIANT HEAT IS AN EXCEPTION TO THE SECOND LAW OF THERMODYNAMICS

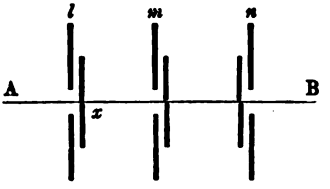
[From the *Scientific Proceedings of the Royal Dublin Society.*
Read December 18, 1882.]

In the Scientific Proceedings of the Ohio Mechanics' Institute for July, 1882, and at the meeting of the American Association for the Advancement of Science in Montreal, Dr. Eddy proposed an exceedingly ingenious experiment, by which he expected that it would be possible to get radiant heat to pass from a cooler to a hotter body. I had the pleasure of hearing his paper at Montreal, and though I then saw no flaw in his reasoning, I pointed out to him that he had only taken two of the bodies concerned into consideration, yet I dared not express a decided opinion on the matter without further consideration. The further consideration has resulted in my thinking that the reasoning is not valid, as I hope to be able to point out.

I shall take what seems to me an experiment somewhat simpler in theory, though one that would be harder to carry out in practice, than Dr. Eddy's.

Suppose that two regions A and B are separated by three parallel screens l, m, and n, having apertures in them x, y, z, capable of being opened and closed from the centre, so as to make everything perfectly symmetrical round the line AB perpendicular to the screens. Now, if x be opened for a very short time, a certain quantity of radiant energy will escape out of A into the region between l and m; and if y be opened when this heat reaches m

it can certainly be let on into the region *mn*; and if *z* be similarly opened when it reaches it, this radiant heat will get into B. While *z* was open, however, some heat left B; but as Dr. Eddy observes, *y* may be closed so as not to let this ever get through the screen *m*, and it can be all returned into B by reflection through *z* or some other aperture. So far I entirely agree with Dr. Eddy, and so far it seems as if the result had been to transfer heat from A to B without B's losing any heat by having it transferred to A. As I warned Dr. Eddy when I heard his paper, there are, however, other bodies and regions to be considered besides A and B. There are more than two bodies considered: there is the region of the screens. Consider what happens when the heat that escaped out of B into the *mn* region tries to get back into B. Some door must be open to let it pass,



and, while it is passing in, an at least equal amount of heat will be passing out of B into the *mn* region, so that you can never really get the heat that has once left B back into B again. This is true whether you adopt doors over fixed apertures such as I have supposed, or moving apertures such as Dr. Eddy proposed. What really takes place is this: a certain quantity of heat escapes out of A and reaches B, and a not less quantity of heat leaves B and is kept entangled in the region of the screens, and it is only possible to let the heat pass from A to B by means of this third region. Hence this only really comes to the same thing as letting A radiate some of its heat into the screen region while B is kept closely shut up. Now be it observed that Dr. Eddy practically postulates that this screen region is at least colder than A—in fact he assumes it to be perfectly cold, *i.e.* to contain no radiant heat except what is admitted from A

and B, so that it is by no means contrary to the theory of exchanges that A might cool by radiating into this region.

This seems to me to be a perfectly satisfactory explanation of what would take place, though it, for the present, dispels the vision of an immortal universe. This experiment of Dr. Eddy's, however, seems to call attention to the fact that a region of motionless ether must be considered as practically a cold body capable of absorbing radiations.*

[* This subject is now more fully analyzed and understood, through the work of Bartoli, Boltzmann, W. Wien, Rayleigh, and others; for a brief discussion, to which Prof. Fitz Gerald contributed, see Larmor, *Brit. Assoc. Report*, 1900, reprinted *infra*.]

24

ON THE QUANTITY OF ENERGY TRANSFERRED TO THE ETHER BY A VARIABLE CURRENT

[From the *Transactions of the Royal Dublin Society* (N. S., Vol. III). Read November 19, 1883.]

I TAKE the simple case of a small circular current varying according to the simple periodic pendulum law.

If the current be a small circle of radius a round the origin in the plane xy, then the direction of the electromagnetic potential ($\mathfrak{A}$) at any point is evidently in the circumference of a circle through the point parallel to the current, and whose centre is on the axis of z, and its amount is

$$\mathfrak{A} = \mu a \int \frac{i \cos \theta d\theta}{r},$$

when θ is the angle between the element of current $ad\theta$ and the direction of the potential at the point, and i the intensity of the current.

This value is not the complete value when i varies, because there are then displacement currents due to the variations of electric displacement which produce electromagnetic potentials of their own. The complete value of $\mathfrak{A}$ must, however, be such as to satisfy the equation

$$\Delta^2 \mathfrak{A} + K\mu \ddot{\mathfrak{A}} = 0,$$

for all points outside the current, and must vanish at infinity.

Now, if i be simply periodic, we may assume

$$i = i_0 \cos 2\pi \frac{t}{T},$$

and if we take

$$\mathfrak{A} = \mu a i_0 \int \frac{\cos \frac{2\pi}{T}(t - \sqrt{K\mu}\, r)}{r} \cos\theta d\theta,$$

it, as a function of r, satisfies the required conditions, and for small values of r and large values of T is the same as if we neglected the displacement currents.

In order to get the energy of the ether due to the displacement currents in it we must calculate

$$T = \tfrac{1}{2} \iiint (Fu + Gv + Hw)\, dxdydz = \tfrac{1}{2} \iiint S(\mathfrak{A}\mathfrak{C})\, dxdydz.$$

Now, as $\mathfrak{C} = -\frac{K}{4\pi}\dot{\mathfrak{A}}$ in this case, and as $\dot{\mathfrak{A}}$ is parallel to $\mathfrak{A}$, inasmuch as $\dot{\mathfrak{A}} = -\frac{4\pi^2}{T^2}\mathfrak{A}$,

$$T = -\frac{K}{8\pi} \iiint \mathfrak{A}\dot{\mathfrak{A}}\, dxdydz = \frac{K\pi}{2T^2} \iiint \mathfrak{A}^2\, dxdydz$$

$$= \frac{K\mu^2\pi}{2T^2} (ai_0)^2 \iiint \left(\int \frac{\cos \frac{2\pi}{T}(t - \sqrt{K\mu}\, r)}{r} \cos\theta d\theta \right)^2 dxdydz.$$

In order to calculate this we must calculate the value of

$$\left(\int \frac{\cos \frac{2\pi}{T}(t - \sqrt{K\mu}\, r)}{r} \cos\theta d\theta \right)^2$$

$$= \left(\cos\frac{2\pi t}{T} \int \frac{\cos \frac{2\pi}{T}\sqrt{K\mu}\, r}{r} \cos\theta d\theta + \sin\frac{2\pi t}{T} \int \frac{\sin \frac{2\pi}{T}\sqrt{K\mu}\, r}{r} \cos\theta d\theta \right)^2.$$

Now as all we require is the average energy of the vibration, it evidently depends on the part independent of the time in this, *i. e.* on

$$\left(\int \frac{\cos \frac{2\pi}{T}\sqrt{K\mu}\, r}{r} \cos\theta d\theta \right)^2 + \left(\int \frac{\sin \frac{2\pi}{T}\sqrt{K\mu}\, r}{r} \cos\theta d\theta \right)^2.$$

If we call $\frac{2\pi\sqrt{K\mu}}{T} = l = \frac{2\pi}{\lambda}$, when λ is the wave-length, we require to calculate the values of

$$\int \frac{\cos lr}{r} \cos\theta d\theta, \quad \text{and of} \quad \int \frac{\sin lr}{r} \cos\theta d\theta.$$

In order to do this I shall assume a the radius of the current small, and take R the distance of the point from the centre of the current, and p its distance from the axis of z; then

$$r = (R^2 - 2ap\cos\theta + a^2)^{\frac{1}{2}} = R\left(1 - \frac{ap}{R^2}\cos\theta\right);$$

therefore $$\cos lr = \cos lR + \frac{lap}{R}\sin lR\cos\theta;$$

and $$\frac{\cos lr}{r} = \frac{\cos lR}{R} + \frac{ap}{R^3}\cos\theta(\cos lR + lR\sin lR).$$

Similarly, $$\frac{\sin lr}{r} = \frac{\sin lR}{R} + \frac{ap}{R^3}\cos\theta(\sin lR - lR\cos lR);$$

therefore, $$\int_0^{2\pi} \frac{\cos lr}{r}\cos\theta d\theta = \pi\frac{ap}{R^3}(\cos lR + lR\sin lR),$$

$$\int_0^{2\pi} \frac{\sin lr}{r}\cos\theta d\theta = \pi\frac{ap}{R^3}(\sin lR - lR\cos lR).$$

Hence the sum of the squares of these integrals is

$$= \pi^2 a^2 \frac{p^2}{R^6}(1 + l^2R^2).$$

So that the average value of T is*

$$T = (\pi a^2 i_0)^2 \frac{\pi K\mu^2}{2T^2} \iiint (1 + l^2R^2)\frac{p^2}{R^6}\,dx\,dy\,dz.$$

If this be integrated over the sphere of which R is a radius, it will give the average energy on that sphere.

[* The average is half this, on account of the alternation; but as there is also an equal average amount of potential energy, the formulæ for the total energy which follow are correct. See p. 128.]

Now $p = R \sin \phi$, $dx\,dy\,dz = R^2 \sin \phi\, d\phi\, d\psi\, dR$.
So that if we assume R constant, the average superficial energy is

$$= (\pi a^2 i_0)^2 \frac{l^2 \mu}{8\pi} (1 + l^2 R^2) \frac{4\pi}{3R^2}$$

$$= (\pi a^2 i_0)^2 \frac{l^2 \mu}{6} \left(l^2 + \frac{1}{R^2} \right).$$

Now, one part of this is independent of the size of the sphere, while the other part varies inversely as the square of the distance from the current. It is evidently only the first part of the energy that is really radiated; and if we assume that it moves with the same velocity as the waves, $= \frac{1}{\sqrt{K\mu}}$, we find that the energy radiated per second is

$$(\pi a^2 i_0)^2 \frac{l^4 \mu}{6\sqrt{K\mu}}; \text{ or as } l = \frac{2\pi \sqrt{K\mu}}{T} \text{ if } V = \frac{1}{\sqrt{K\mu}}$$

it is

$$(\pi a^2 i_0)^2 \frac{8\mu\pi^4}{3T^4 V^2}.$$

Except for the coefficient $\frac{8}{3}\pi^4$, this value can be got by considering the dimensions of the quantities involved, if we assume that the energy radiated is proportional to the square of the moment of magnetism of the radiating particle ($\pi a^2 i_0$), which is evidently the case from the value of the energy in the form $Fu + Gv + Hw$, as F, G, H and u, v, w are all proportional to this moment.

If we calculate the amount of this energy we find that it is very small, except for very rapid vibrations indeed. For

$$\frac{8\pi^4}{3} = 260 \text{ q. p.},$$

while $V^2 = 26 \times 10^{20}$; therefore $\frac{8\pi^4}{3V^2} = 10^{-19}$ q.p.;

and as μ generally equals unity in electromagnetic measure, the energy radiated $= (\pi a^2 i_0)^2 \times N^4 \times 10^{-19}$, where N is the number

of vibrations per second, so that even if it were as many as ten million per second (10^7), the energy radiated would still be only $\frac{1}{10}(\pi a^2 i_0)^2$ ergs per second.

If we knew what the radiating power of a gas were, and assumed that its molecules gave their energy to the ether according to the same laws as varying electric currents do, we might estimate the magnetic moment of what corresponds to electric currents in a molecule.

The energy in the neighbourhood of the radiating point that I rejected as varying with the distance from it, and consequently as unconnected with the radiation, is what would be got if we neglected the effects of induction of the displacement currents; *i. e.* it is the energy of the forced displacement currents produced directly by the variation of the primary current, and which start the radiating displacement currents. This part of the whole energy in the medium is but a small part of that at any point except within a few wave-lengths of the origin.

25

ON MAXWELL'S EQUATIONS FOR THE ELECTRO-MAGNETIC ACTION OF MOVING ELECTRICITY

[From the *British Association Report*, 1883. See Nos. 20, 22 *supra*.]

MAXWELL only once mentions this action in his "Treatise on Electricity and Magnetism," in § 768, although it is just as necessary as his displacement-currents in order to be able to consider all circuits as closed.

The equations for the electromagnetic field, § 618, are incomplete, for he introduces as the coefficient of c in the equations for the mechanical force $-\frac{d\psi}{dx}$, $-\frac{d\psi}{dy}$, and $-\frac{d\psi}{dz}$ in its three components, while it would be more complete to have put in P, Q, R, the complete components of the electromotive force. By so doing he would have introduced the terms $c(c\dot{y}) - b(e\dot{z})$, which are evidently the terms expressing the convective action. Maxwell himself practically makes this substitution in § 631, and deduces indirectly his electromagnetic theory of light from the term $-e\frac{dF}{dt}$ thereby introduced.

26

ON THE ENERGY LOST BY RADIATION FROM ALTERNATING ELECTRIC CURRENTS

[From the *British Association Report*, 1883. See No. 24 *supra*.]

I TAKE the simple case of a small circular current. The components of the vector potential at any point must satisfy $\Delta^2 F + \kappa\mu\ddot{F} = 0$, while for points very close to the elements of currents they must be $F = \mu\frac{dc}{r}$. Assuming the current simply periodic, $c = c_0 \cos 2\pi \frac{t}{T}$, then

$$F = \mu \int \frac{c_0 \cos \frac{2\pi}{T}(t - \sqrt{K\mu}\, r)}{r} \frac{dx}{ds}\, ds.$$

The energy in any element of the field* is per unit volume $Fu + Gv + Hw$, and as $u = -\frac{K}{4\pi}\ddot{F}$, &c., we can easily represent in terms of the square of the above integral the energy per unit volume. Estimating it for the case of a very small circular current, it gives for the energy at any time on a sphere of radius R,

$$E = (\pi a^2 c_0)^2 \frac{\mu}{6} e^2 (e^2 + R^{-2}),$$

where a is the radius of the small circuit, and e is $\frac{2\pi\sqrt{K\mu}}{T}$.

[* Being in a time-average half kinetic and half potential.]

The part of this, independent of the radius of the sphere, is evidently the radiated energy; and, assuming it to move with the velocity of the waves, we find the energy radiated per second

$$e = (\pi a^2 c_0)^2 \frac{8\mu\pi^4}{3T^4V^3},$$

where V is the velocity of wave-propagation $= \frac{1}{\sqrt{K\mu}}$. This is very small indeed, unless the period T be excessively small.

27

ON A METHOD OF PRODUCING ELECTROMAGNETIC DISTURBANCES OF COMPARATIVELY SHORT WAVE-LENGTHS

[From the *British Association Report*, 1883.]

THIS is by utilising the alternating currents produced when an accumulator is discharged through a small resistance. It would be possible to produce waves of as little as 10 metres wave-length, or even less.

28

NOTE ON DUST REPULSION

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read June 5, 1884.]

DR. OLIVER J. LODGE has pointed out that when a body is warmer than dusty air in its neighbourhood, the dust has a tendency not to settle on it. When speaking to him on the subject he did not seem to have been aware of Professor O. Reynolds' experiments on the motion of silk fibres suspended in air and subject to radiation,* which Professor O. Reynolds explains by the absorption of the radiations by the silk, and the consequent polarization of the air, warmed by contact with the small fibres, producing an apparent repulsion of the fibre. Professor O. Reynolds' theory of the radiometer points to the conclusion that very small bodies would act in dense air in the same way as the comparatively large vanes of a radiometer do in rare air. This application of Professor O. Reynolds' theory to explain the tendency of the warmth of a body to prevent dust settling on it seemed very obvious, and occurred to me immediately I heard of the experiments of Dr. Lodge, and I would not have thought it worth while mentioning only that I found that Dr. Lodge himself did not seem to have noticed it.

* Phil. Trans. R. S., 1879, part ii. "On Certain Dimensional Properties of Matter in the Gaseous State."

29

ON CURRENTS OF GAS IN THE VORTEX ATOM THEORY OF GASES

[From the *Scientific Proceedings of the Royal Dublin Society.* Read June 5, 1884.]

MR. J. J. THOMSON, in his book on "Vortex Atoms," has called particular attention to the fact that, the momentum of the vortex ring being proportional to its area, and its velocity through the medium getting less the greater its momentum, the velocity of hot simple vortex atoms is less than that of cold ones. Professor Osborne Reynolds, in his review in *Nature*, Decem. 27, 1883, of Mr. J. J. Thomson's book, called attention to the discrepancy that exists between this result and the fact that the velocity of sound is greater in hot than in cold gas. Now, on considering how the velocity of sound in a gas composed of vortex rings would be propagated, it is at once evident that the usual equations of motion do not apply to a system of atoms whose momentum is not proportional to their velocity, and this at once raises the question, What is the momentum of a current of vortex atoms? The answer to this of course depends on what the nature of a current of gas formed of vortex atoms is supposed to be. If it be supposed to be a current of the medium carrying the vortices with it, and if it be further supposed that the amount of medium carrying a given mass of vortices is independent of their size, then the momentum of the current will be, as we know it is, independent of the temperature of the gas. These assumptions, however, seem quite at variance with Mr. J. J. Thomson's investigation as to the relative velocity of

hot and cold vortex atoms, where he assumes that each vortex atom moves, independently of the rest, through a medium at rest, and carries its momentum with it, and that the general effect is simply the superposition of the effects due to these independent vortices. The assumption as to the nature of a current of gas, that is consistent with this method of treating the subject, is the usual assumption that a current of gas consists in the motions of the molecules being so polarized that there are a greater number of molecules moving in the direction of the current at any one time than in any other direction, *but that the molecules are moving individually independently, and in the same manner as they do in a gas at rest.* For instance, if a region in which there were many vortex atoms were in communication with one in which there were only a few, the vortex atoms would diffuse in a current out of the former into the latter. Now the momentum of this current for a given number of vortex atoms transferred per second across unit area would depend on the momentum of each vortex atom, and as this momentum is not proportional to the velocity of the atom, and is a function of its temperature, it is evident that the momentum of such a current would be a function of its temperature.

The whole notion that atoms are vortex rings in a medium raises serious difficulties as to the amount by which this medium is carried along by the motion of the Earth, for instance, and by solid bodies enclosing masses of gas. If the Earth does not carry the medium with it, the vortex atoms in the air must be so energetic as to be moving at nineteen miles a second through it, and their size and momentum must vary considerably from day to night, as they are going so much more rapidly at night than by day. Similarly the momentum of an enclosed mass of gas would vary with its temperature, if moving the enclosure only moves the enclosed vortices. On the other hand, if the Earth does carry the medium with it, what withstands the centrifugal force of this revolving medium? Why does it not originate an enormous vortex? This and similar difficulties seem to point to the conclusion that to suppose atoms to be simple ring vortices

in a perfect liquid can hardly be an adequate theory. There seems very good reason for believing that the hypothesis that the medium is simply a perfect liquid is hardly sufficient. It certainly is not sufficient to explain luminous and electrical and magnetic phenomena, to suppose the ether to be simply a perfect liquid at rest; and though it is too much to say that it will not ultimately be found a sufficient foundation for a theory of matter and material phenomena, at present, at least, there seems very little prospect of anybody's inventing how it ought to be moving, in order to have the known properties of the ether; and even the ingenious attempt to explain matter as due to simple ring vortices in it seems too simple to be adequate.

30

ON A METHOD OF STUDYING TRANSIENT CURRENTS BY MEANS OF AN ELECTRODYNAMOMETER

[From the *Scientific Proceedings of the Royal Dublin Society.* Read June 7, 1884.]

WHEN a current of short duration, compared with the period of free vibration of a galvanometer needle, is passed through the galvanometer, the total quantity of the current is proportional to the sine of half the angle of swing of the needle. The swing produced is independent of the distribution of intensity of the current during its passage.

Now, if a similar transient current be passed through an electrodynamometer, we have the following equation for the motion of the suspended coil while a current C is passing:—

$$I\frac{d^2\theta}{dt^2} = KC^2,$$

where I is the moment of inertia of the coil and K a constant.

Hence $$\left.\frac{d\theta}{dt}\right|_0 = \frac{K}{I}\int C^2 dt$$

gives the angular velocity with which the needle begins to swing. If the suspension be bifilar during its free swing the motion is given by the equation

$$I\frac{d^2\theta}{dt^2} = -k\sin\theta,$$

where k is a constant depending on the method of suspension, &c. Hence, for the maximum swing,

$$\left.\frac{d\theta}{dt}\right| = 2\sqrt{\frac{k}{I}}\sin\tfrac{1}{2}\theta.$$

Comparing this with the other value, we obtain

$$\int C^2 dt = \frac{2\sqrt{Ik}}{K} \sin \tfrac{1}{2}\theta.$$

Now the value of $\int C^2 dt$ depends upon how the current varies during its period of transit, and if we can form any hypothesis as to the law of its variation, we can calculate the period it took to pass, and its maximum electromotive force by comparing this result with that given by an ordinary ballistic galvanometer.

For instance, the secondary current at any time in Ruhmkorff's coil may, to a first approximation at least, be represented by an equation

$$C = C_0 \sin \pi \frac{t}{T},$$

where C_0 is the maximum current, and T the whole time of flow. The total current as given by the balistic galvanometer is

$$Q = \int C dt = C_0 \int_0^T \sin \pi \frac{t}{T} dt = \frac{2TC_0}{\pi}, \text{ so that } C_0 T = \tfrac{1}{2}\pi Q;$$

the quantity given by the electrodynamometer is

$$S = \int C^2 dt = C_0^2 \int_0^T \sin^2 \pi \frac{t}{T} dt = \tfrac{1}{2} C_0^2 T;$$

therefore $$T = \frac{\pi^2 Q^2}{8S}, \text{ and } C_0 = \frac{4S}{\pi Q}.$$

It is thus easy to see that the comparison of these observations would give a great deal of information about transient currents.

31

ON A NON-SPARKING DYNAMO

[From the *Scientific Proceedings of the Royal Dublin Society.* Read June 9, 1884.]

WHAT are called unipolar dynamos have no sudden change in the nature of the sliding contact, which is essential in all dynamos, and are consequently non-sparking. The arrangement I am about to describe is somewhat different, and is an application to a dynamo of the principle that Maxwell applied to Sir William Thomson's electric doubler (Maxwell's "Electricity and Magnetism," vol. i, p. 298, s. 213) in order to prevent sparks in it, and which he likened to the regenerator in a heat engine.

Consider a series of coils arranged to rotate in a magnetic field as a gramme ring, but with their wires unconnected with one another, each coil being connected with a separate commutator and pair of brushes, and half the brushes being connected with one electrode, and the other half with the other. In this case the current would in general go through a number of the coils in parallel circuit. Now it is evident that by a suitable arrangement of the magnetic field and of the positions of the brushes on the commutator, it could be arranged that the break of contact of the brushes and commutator should take place when the current in the coil was zero. This is evident, because if the brushes made contact throughout a whole revolution, as in an alternating current dynamo, the current would be reversed every half revolution; and if the instant of reversal were the instant of break of contact, this would happen when no current was passing in the coils. It is not possible to arrange this

unless there are at least two parallel coils carrying the current, as otherwise the whole current would be fluctuating, and might as well be alternating. With a number of coils parallel, it would evidently be quite possible to arrange that the time of break of contact of each pair of brushes and its commutator should occur when no current was running in the corresponding coil. One way of doing this would be, as in Maxwell's electric doubler, to arrange a subsidiary magnet system to alter the magnetic field until the desired effect was produced. In any arrangement, with a number of coils in parallel circuit, it would be necessary to be particular as to the arrangement of the magnetic field, for fear of the currents being reversed in some of the coils. This would evidently waste a good deal of energy in heating the coils. In the proposed arrangement the energy of self-induction usually wasted in sparks would be spent in keeping the current running in the line, and there is reason to expect that such a dynamo would not exhibit the apparent increase of resistance with speed pointed out by Messrs. Ayrton and Perry.

32

ON AN ANALOGY BETWEEN ELECTRIC AND THERMAL PHENOMENA

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read November 17, 1884. See No. 5.]

THE object of the following communication is to show how little we know of the nature of what are called electric currents, by pointing out that electric quantities may be described in terms of temperature and entropy just as satisfactorily as in terms of pressures and volumes, which are the terms analogous to electric potential and quantity of electricity in consonance with the usually drawn analogy between an electric current and a current of an incompressible fluid, upon which is founded such terms as electric current and electric displacement. The thermal analogy usually drawn is between quantity of electricity and quantity of heat; but I think the analogy I am pointing out is in every way preferable, for energy is of the nature of temperature by entropy, just as it is of the nature of pressure by volume or potential by quantity of electricity. The work done in bringing a quantity of entropy, ϕ, from temperature T_1 to T_2 is $W = (T_1 - T_2)\phi$; and if the transference be effected in a reversible manner, the entropy taken from one source is equal to that given to the other. Indeed we can easily conceive of systems in which all transference of entropy is effected without loss, and in that case the quantity of entropy would be constant. It is to be observed that the temperature and entropy of a body are quite independent of one another, so that we can suppose either to be altered while the other remains constant. If we apply this conception to

electrostatic phenomena, we must call potential the temperature of the ether and quantity of electricity its entropy. As the same relations connect these pairs of analogues with energy they will obey similar laws, and what is further required is to show how it may be supposed that bodies at different temperatures can throw the intervening medium into the sort of stress that Maxwell has shown is required to explain electrostatic forces. I premise that Maxwell has not made any attempt, in his "Electricity and Magnetism," to make the corresponding step of showing what sort of a thing an electric displacement is that it may produce these stresses, so that the line I am pointing out goes beyond Maxwell's analogy, but must be considered as only a dim sort of hypothesis, and not as a complete theory.

In the first place, it is evident that a non-conductor must be a medium that can have different temperatures at different parts without entropy being conducted from one place to another, *i. e.* without conduction of energy; and a conductor must be an engine that transfers entropy undiminished, and always produces heat or work equivalent to the degradation of energy.

Now a distribution of molecules of a gas and their velocities is possible by which there would be stresses in the gas similar to those required by electrostatic phenomena, and yet without either currents of gas or conduction of heat taking place. The molecules of a material gas do not get themselves or their velocities distributed in this way by differences of temperature: in all material gases there is certainly conduction of heat, but it seems probably possible to invent a law of action between molecules which shall produce any required law of distribution of molecules and their velocities. A somewhat similar problem was solved by Maxwell when he calculated the law of force between molecules, in order that the temperature at all points in a column of gas subject to gravity might be the same. As the whole thing is so hypothetical, I have not thought it worth while attempting to work out the law of action between molecules that would account for the distribution of velocities required by my hypothesis.

If the number of molecules going in a direction defined by a direction-angle θ be represented by ($\mu = \cos\theta$)

$$m = m_0 + m_2(\mu^2 - \tfrac{1}{3});$$

and if the velocity of the corresponding molecules be

$$v = v_0 + v_2(\mu^2 - \tfrac{1}{3}),$$

then there will be neither currents of gas nor to the second order of small quantities any conduction of heat, but the pressure will be different in different directions, and will be, parallel to the direction from which θ is measured

$$p_z = P + p,$$

and in every direction at right angles to this

$$p_r = P - p,$$

where*

$$P = \tfrac{1}{3}\pi(v_0 - \tfrac{1}{3}v_2)\{m_0v_0 + \tfrac{1}{3}m_0v_2 - \tfrac{2}{3}m_2v_2 + \tfrac{1}{15}(m_2v_0 + 2m_0v_2 - m_2v_2)\},$$

and

$$p = 1\tfrac{2}{5}\pi(v_0 - \tfrac{1}{3}v_2)\{m_2v_0 + 2m_0v_2 - m_2v_2\}.$$

If in this m_2 and v_2 be negative quantities, p will be negative, and the state of stress will be a pressure less than the average along the line from which θ is measured, and an equal excess above the average pressure in every direction at right angles to this; and this is the state of stress that Maxwell has shown will explain electrostatic forces.

If we take the particular case of two parallel planes, and suppose P and p constant all the way across, as they evidently must be, we have, supposing $m_2 = 0$, that $m_0v_0^2$ and $m_0v_0v_2$ must be constant as we go across, so that v_0 and v_2 must vary inversely as the square root of m_0; and if we suppose temperature defined by the square of the velocity, *i.e.* by v_0^2, we have that it must be of the form $T_0 + T_1r$, where T_0 and T_1 are constants, and r the distance from one of the planes.

In this method of describing electrostatic phenomena, what Maxwell calls the electric displacement would be proportional to the square root of p; and what he describes as the polarization of the medium I would describe in the same words, and explain

[* These formulæ seem to require alteration.]

as a distribution of the molecules and their velocities in which these were not uniformly distributed in every direction.

I have not propounded any hypothesis as to the nature of conduction, but it would evidently correspond to a transference of entropy by means of heat engines which would produce heat corresponding to the resistance of the conductor, and also produce magnetic phenomena in the neighbouring dielectric. I have not propounded any hypothesis as to the nature of magnetic displacements, but it seems probable that they might be illustrated by introducing molecular rotation.

I am bringing forward this communication not at all so much for the sake of the hypothesis it contains as for the sake of calling attention to the purely analogical nature of the description of electric currents as currents, and electric displacements as displacements. The analogy between electric currents and currents of an incompressible fluid is so complete and so easily drawn, that there is serious risk of our being as much hampered in future advances by our imagining this analogy to be a likeness as we have been in the past by the theories of action at a distance, which still control the text-books and the ordinary methods of speaking of electrical phenomena to such an extent that most people find it almost impossible to think of electric and magnetic phenomena in any other light. For fear of our being similarly hampered by the words "electric displacement" and "electric current," I think it worth while pointing out that just as these words presuppose an analogy between electric quantity and volume of liquid, and electric potential and pressure of liquid, similarly it is quite possible to draw another analogy, namely between electric quantity and entropy, and electric potential and temperature, so that with these two different analogies in view, it may be the less likely that the student should look upon either as anything more than an analogy, until further investigation shows that there is either a real likeness to one or the other, or that the electric phenomenon is *sui generis*, and unlike either, which latter is in my opinion the more probable supposition.

33

ON A MODEL ILLUSTRATING SOME PROPERTIES OF THE ETHER

[From the *Scientific Proceedings of the Royal Dublin Society.*
Read January 19, 1885. See the Introduction.]

THE model I have constructed consists of a series of wheels, rotating on axes fixed perpendicularly in a plane board, and connected together by indiarubber bands. The axes are fixed at the intersections of two systems of perpendicular lines, and each wheel is connected with each of its four neighbours by an indiarubber band. Thus all the wheels can rotate without any consequent straining of the system if they all rotate at the same rate. If, however, some of the wheels be rotated through a different angle from others, the indiarubber bands will be strained.

If it is desired to represent a region in which conducting matter exists, it will be represented by removing the bands from a set of wheels. Suppose the bands are removed from the regions a and b, and from the connecting line ab, we can represent the charging of these regions with opposite electricities by introducing some mechanism, by means of which the wheels on opposite sides of the line ab can be rotated in opposite directions. And now I desire to explain that I do not intend the model to illustrate at all the connexion between the ether and the matter, and indeed think it one of the advantages to be derived from studying this model that it so distinctly emphasizes the distinction between the phenomena, depending on the general properties of the ether by itself, and those depending on its

connexion with matter. For instance, from this very case we are considering, we get impressed upon us that it is by means of matter only that we can get a hold on the ether so as to strain it. As I am not careful to illustrate the connexion between matter and ether, any rough method of turning the wheels so as to create the proper strain will do well enough, as it is not the method of producing but the nature of the strain produced that I wish to study. Having once rotated these wheels, we may replace the bands along the line *ab*, and we have the state of the ether between two oppositely electrified bodies represented on the model. Observe that half the indiarubber bands are strained, and that in lines running round the bodies the tight side of a band is always away from one body and next the other. This represents the polarization of the ether. Clerk Maxwell defines polarization as a state in which the opposite sides of each element are in opposite states. Now the opposite sides of each band are in opposite states, one side loose, the other tight; and so it can very well represent the polarized state of the ether. The displacement producing the polarization is due to the different rotation of the wheels carrying the band causing more of the band to be at one side of the wheels than the other: less at the tight and more at the loose side of the pair of wheels, and this represents the electric displacement producing the polarization. The direction of this displacement is at right angles to the line of the bands that are strained, and is out from one body and in towards the other, all round. Now, one of the first things one would expect a model of the ether to represent would be the force of attraction between these two electrified bodies, and which is not represented on my model. This, however, is because this force depends entirely on the connexions between the ether and matter; and as I have already explained, this connexion is not represented on my model, and in consequence of this the attraction depending on this connexion is quite rightly not represented on my model. I may, however, be permitted to suggest a way in which my model might be modified so as to represent this force, especially as it

will emphasize a point about this force that is frequently overlooked in describing electrical phenomena. This point is that the force is proportional to the square of the electric displacement, and consequently cannot be directly and simply due to it, as in ordinary solids forces are produced by displacement: for in them the force is proportional to the displacement, and not to its square. The result is that the sign of the force is independent of the sign of the displacement, and is the same whether it be positive and negative, or *vice versa*. This is a very important distinction, and is obvious when pointed out, for it is evident that it is electromotive force, and not the mechanical force, that depends on electric displacement in the same way as mechanical forces depend on displacements of solids.

Now, if the wheels over which the bands run were made of some deformable, not merely incompressible, substance, such as indiarubber, it is evident that they would be deformed by the straining of the bands. Now, if inextensible threads were wound round them, and the ends of the threads connected respectively with the bodies a and b, it is evident that, as the deformation of the circular wheels would enlarge their circumferences, these threads would tend to draw the two bodies together. Now, the shortening of the threads would be proportional to the square of the linear deformation of the wheels, and so the resulting force might represent the electrostatic attraction of the two bodies. If we consider the force that would be thus produced between two infinite planes, we see that it would be the same no matter what their distance apart so long as the polarization is kept the same; and that consequently it will represent a force varying inversely as the square of the distance from a point. I mention this crude mechanism in order to show that it is possible to represent by mechanism the connexion between the ether and matter, and also to illustrate how electrostatic attractions depend entirely on the way the ether is connected with matter.

Let us now return to consider what other properties of the ether are represented by the model. Observe, in the first place,

that during the time polarization is taking place the wheels are rotating, and that the rate of rotation of the wheels is proportional to the rate of increase of polarization, and that the direction of the axis of rotation is perpendicular to the direction of the displacement. We, of course, at once conclude that the magnetic force is properly represented by the rate of rotation of the wheels, and its direction by the axis of rotation. We thus see that any change of displacement produces rotation of the wheels, *i. e.* [magnetic force; and we will presently see that what represents an electric current is similarly accompanied by rotation of the wheels. The momentum of the wheels while rotating represents the kinetic energy of the currents producing the magnetic force, and in the case of one current is its self-induction. Now, if we suppose something to loosen the hold of the bands on the wheels, this will represent the presence of some partially conducting matter, for evidently perfectly conducting matter must be represented by the fact that no amount of difference of rotation of neighbouring wheels, *i. e.* no amount of electric displacement, can produce any polarization, and we have represented it by removing the bands from the wheels entirely: a conductor offering some resistence would be represented by making the bands loose. If the bands be not sufficiently tight on the wheels between *a* and *b*, they will slip if the polarization be carried far enough; and if the kinetic be much less than the statical friction, the polarization of the medium will suddenly disappear by the slipping of the bands, and the energy of the polarization will be converted into heat along the line of slipping. This evidently represents a discharge between the two electrified bodies. Now, observe further that, if the resistance to discharge be very small, the momentum of the wheels will carry them beyond their position of complete depolarization, and a series of oscillations will be set up, the polarization alternating in opposite directions; but, of course, ultimately all the energy will disappear as heat along the line of discharge. This exactly represents the discharge between two electrified bodies. If the resistance to discharge be small

enough, an oscillating discharge is produced which, however, ultimately dies away, all its energy being spent on heating the wire along which the discharge occurs. Now, observe further, the direction in which the energy of the medium flows in to the line of discharge. It comes in in the length of the indiarubber bands, *i. e.* in a direction at right angles both to the electric displacement and to the magnetic force, and it comes in at the side of the line of discharge. This is exactly what Professor Poynting has lately shown to be the case in the ether.

Let us now consider the case of a complete conducting circuit in which there exists an electromotive force at one place, and resistances all round. This can be represented by taking away the bands, or making them loose, along a series of wheels in a circuit, and arranging mechanism for turning the wheels at one part of this circuit, and introducing some friction at all the other parts. Suppose our electromotive force is along the line *ab*, and that the resistance is introduced by friction along the outside wheels. As I have already explained that I do not intend my model to illustrate the action between matter and ether, any crude mechanism suffices to represent the electromotive force. The circuit where there are no bands, or loose ones, is our conducting circuit, and the friction to the wheels along it is the resistance of the circuit. Now, if the wheels be driven at any point of the circuit, all the region will be full of wheels turning, for all the wheels must turn simultaneously. This rotation of the wheels represents the magnetic force, and the motion of the parts of the wheels in contact with the circuit being everywhere in the same direction round it, represents the electric current. There is no transference of anything along the current, and the energy developed at any point is transferred to that point, not along the current, but in at its side in the direction of the elastic bands perpendicular to the current. Now, this is exactly the state of affairs that Professor Poynting showed to exist in an electric current. This direction is at right angles to the axis of rotation of the wheels, and also to the direction of polarization of the bands, *i. e.* to the magnetic and

electric displacements. The self-induction of this circuit is represented by the momentum of the wheels. It is easy to see how the polarization of the bands is connected with the resistance along any length of the circuit. When the resistance is great the bands will be greatly polarized, *i. e.* the electric displacement is great, *i. e.* the rate of fall of potential will be great; and as this will be directly proportional to the resistance, we see that the fall of potential along any length of the circuit is proportional to the resistance of that section, which is Ohm's Law. In order completely to represent Ohm's Law, it would be necessary to arrange that the friction was proportional to the rate of revolution of the wheels. This however, is trenching on the connexion of matter and ether. Another question, similarly circumstanced, is the mechanical force exerted on the conductor, due to a magnetic field. It is evident that the direction in which to look for a mechanism to represent this would be something depending on the centrifugal force of the rotating wheels; but I have not invented any satisfactory way of representing it. The mutual induction of currents may be exhibited by making two circuits, on one of which the current is forced, and on the other of which the bands are simply loose. Now, on starting the first circuit, the wheels outside the second all rotate the same way, which I have explained represents an electric current in it; and the wheels inside will stay unmoved until the friction of the loose bands gradually sets them in motion, and thus, after a little while, the wheels all over the region are rotating just like those anywhere else, *i. e.* those at opposite sides of the circuit are rotating in the same direction, and the current has ceased in the circuit. The opposite effect will, of course, take place on stopping the primary current.

I have thus shown how the states of the ether that Clerk Maxwell assumes, in order to explain electrical and magnetic phenomena, can be illustrated by my model. It is notdifficult either to show that the energy of the ether can be represented by equations exactly the same as those by which Clerk Maxwell expresses it. We must, however, bear in mind that my model

is only a one dimensional one, and would require to be supplemented by two others in planes at right angles to itself to represent a space full of ether. To consider, then, what the energy of the electromagnetic field become in one dimension, I will deal with the form to which I reduced it in my Paper on the "Electromagnetic Theory of the Reflexion and Refraction of Light" (R. S. Trans., vol. ii, 1880). The potential energy in an isotropic medium is

$$W = \frac{1}{4\pi K}\iiint\left[\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right)^2 + \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right)^2 + \left(\frac{d\eta}{dx} - \frac{d\xi}{dy}\right)^2\right] dxdydz;$$

while the kinetic energy is

$$T = \frac{\mu}{8\pi}\iiint\left[\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2\right] dxdydz.$$

Now with only one of the three coordinate planes existing, we must evidently make $\xi = 0$, $\eta = 0$; and if we suppose ζ to be the angular velocity of the wheels, we evidently obtain the right form for the kinetic energy. The potential energy depends on the squares of $+\frac{d\zeta}{dy}$ and $-\frac{d\zeta}{dx}$. Observe that ζ is the angle through which any wheel has rotated; then it is evident that $\frac{d\zeta}{dy}$ is the rate of change of this angle along y, and is proportional to the difference of stretching of the indiarubber bands in this direction, *i.e.* to what I have compared with electric displacement in the x direction; and manifestly, if the bands obey the usual laws of elasticity, their potential energy is proportional to the square of the stretching. We see similarly that the y polarization is $-\frac{d\zeta}{dx}$. It is further evident that, with three systems of planes at right angles to one another, we must make the potential energy depend on the squares of $\left(\frac{d\xi}{dy} - \frac{d\eta}{dz}\right)$, &c., *i.e.* the strain must be proportional to these quantities. Now the simple mechanism of elastic bands will not do this by itself, but it is quite possible to arrange mechanism by which it shall be

effected; for I have been able to invent one working by means of a fluid pumped through a network of pipes, by the rotation of wheels so arranged that an accumulation of fluid in any place tends to stop the rotation. It is easy, then, to arrange that such a series of rotations as $\frac{d\xi}{dy} - \frac{d\eta}{dz} = 0$ shall produce no accumulation. Having shown, then, that the energy of this medium can be expressed in the same form as that of the ether, it follows at once that all the results deduced from this form of the energy can be reproduced on a model.

I have explained how it illustrates ordinary electric phenomena, and now I wish to show how the same medium that can illustrate these phenomena can also transmit vibrations analogous to light. If any series of the wheels be suddenly rotated, inertia will prevent those in their neighbourhood from being immediately rotated. After a little while, however, the motion will be transmitted to the neighbouring wheels, which will transmit it to their neighbours, and so on, a wave of motion being transmitted through the medium. If we consider the nature of the motion constituting the wave, it will be seen to be directed in the plane of the wave, *i. e.* the wave will be a wave of transverse vibration. The axes of rotation of the wheels are evidently at right angles to the direction of propagation of the wave, and what I have described as the direction of polarization of the bands, is also at right angles both to the axes of rotation and to the direction of wave-propagation. This is evident when we consider that the difference of rotation of neighbouring wheels is what causes the tightening of one side of a belt and the loosening of the other; and the direction of polarization of the bands is the line joining the centres of the tight and loose sides of these polarized bands, and this line is evidently at right angles to the line joining the centres of the two wheels whose difference of rotation causes the polarization, while this latter line is evidently the direction of wave-propagation. Now, this is exactly analogous to what Maxwell describes as the nature of the propagation of light in the ether. The axes of rotation

represent the direction of magnetic displacement, while the direction of polarization is the direction of electric displacement. The direction of magnetic displacement is the direction of the plane of polarization of the ray.

From the fact that we can represent a medium in three dimensions, such that its equations of motion are the same as those Maxwell has shown must exist in the ether, it is at once evident that it would be possible to reproduce all the phenomena of the reflection and refraction of light and of polarization of light, and that not merely in ordinary but in crystalline media, so that it would be even possible to reproduce external and internal conical refraction and the other peculiarities of the wave surface. Two related cases may well be noticed—they are those of rotatory polarization by crystalline and magnetized media respectively. The first would obviously be obtained by giving a twist to the planes containing the wheels, a plane polarized ray would then have its plane of polarization twisted during its passage through the medium in the same way as the plane of polarization of a ray of light is twisted in passing along the axis of a crystal of quartz. The phenomena of magnetized media cannot be so easily reproduced. It would evidently require that something should be rotating in the field besides the wheels, and that it and the wheels should be so connected that a rotation of the wheels should tend to change the axis of rotation of this rotating something, which, reacting on the wheels, would change their direction of rotation. Mechanism that did this would reproduce the phenomena of the rotatory polarization of magnetized media. It is worth while remarking how it is necessary, in order to represent this phenomenon, to take into account a change produced by the wave in something besides the wheels, &c., which then reacts on the wave-propagation, so that the action is altogether secondary when compared with a peculiarity of the bands or wheels themselves existing in any region independently of the wave passing through it. This latter state is analogous to the state of the ether inside a body produced by the presence of the matter that causes its refractive

index and specific inductive capacity to differ from unity. The dispersion of waves of different rates of vibration is, on the other hand, a secondary action, as is illustrated by several theories of dispersion where the action of the matter in the wave-propagation is due to a reaction of the matter during the wave-propagation, and not a permanent alteration by the matter of the nature of the medium of propagation.

I need hardly say that I do not intend it to be supposed that the ether is actually made up of wheels and indiarubber bands, nor even of paddle-wheels, with connecting canals. I think, however, that we may learn several things as to the conditions that the elements of the ether should fulfil if they are to represent Maxwell's equations by motions in ways analogous to those of my model. This supposes that quantities, such as the magnetic displacement that obey the laws of rotation, are really of the nature of rotations. If this be so, it is evident that the elements of the medium must be capable of rotating any number of times without any distortional stress being produced, so long as the neighbouring parts all rotate equally, while distortional stress must be produced and be proportional to the difference of rotation of the neighbouring parts of the medium. It seems from this almost necessary that the ether must have some structure, and although Maxwell has objected to its having anything like a molecular structure on account of this supposition requiring it to possess specific heat, yet I venture to think that it does possess the specific heat it should possess upon the supposition of some sort of structure. Part of the energy in a volume occupied by any substance is in the form of vibrations of the ether within that volume, and the amount of this part of the energy will depend on the temperature of the body. Similarly the heat required to warm up a mass of silver enclosed in a non-conducting envelope (so that none of the energy was spent in producing vibrations in the surrounding ether) would depend on whether the silver were all in a small lump, or were in the form of a thin closed vessel, including a large volume of ether, for this included ether would require some of the energy

in order to give it energy of vibration. If the law connecting the temperature of matter with the energy of vibration of the ether in contact with it were known, it would be possible to calculate in this sense the specific heat of ether. As all materials are of the same temperature when in contact with ether in the same state of vibration, this specific heat of the ether would be independent of the material of which the envelope was constructed. As the energy of the ether is all in the form of its own vibrations, and is not propagated by conduction, but by wave-propagation, it is not in exactly the same form as the heat energy in material bodies; but I see no reason for assuming that, because the ether has a structure, it must be capable of being a vehicle for heat energy of exactly the same form as that in material bodies; in fact, unless there were another ether bearing to the known ether the same relation as the known ether bears to matter, it would be impossible that the energy of the known ether should be in exactly the same form as the heat energy in matter. If the medium have a structure we can explain electric polarization much more satisfactorily as a change of structure than as a displacement. Maxwell defines polarization ("Elect. and Mag.," vol. i., § 60) generally thus:—"An elementary portion of a body may be said to be polarized when it acquires equal and opposite properties on two opposite sides." He, however, immediately afterwards calls it electric displacement, and there is danger from this of its being supposed to be merely a displacement of the medium in the direction of the electric displacement. It may be this, but I think it is very desirable that the more general notion of polarization as a change of structure of the elements and not as a displacement of the elements should be emphasised, and it is in the latter form that polarization is exhibited in the model. Another point to which attention is called by the model is the difference between the mechanical and the electrical stress in the medium. The model does not represent the mechanical stress at all. This latter depends necessarily on matter, for mechanical stress is a material phenomenon, and its connexion with the electric

polarization depends on the connexion between matter and ether. This is true of the mechanical forces, due to both electrical and magnetic polarization. That the mechanical stress is not connected with the electric displacement in the same way as material stresses are connected with their material strains in solids is obvious from its being proportional to the square of the displacement, and so being independent of its direction. All theories of the ether that suppose it to be simply a jelly with matter spread through it, like grapes in a jelly, hardly seem to attribute sufficient importance to the difficulty of explaining upon any such simple hypothesis such phenomena as electricity and magnetism; and although the equations of motion of the jelly may fairly well represent the equations of motion of the ether, as regards its propagation of light, yet the properties of a jelly prevent our supposing continuous rotation of its elements, which seems almost necessary in order that the same quantities which represent small motions in the light-propagation may represent known phenomena in electricity and magnetism.

Although Professor Stokes seems to think that there is no contradiction in supposing the ether to be a jelly, and at the same time sufficiently little rigid to permit the free motion of matter through it, nevertheless, there is no doubt that this is a serious stumbling-block in the way of a general acceptance of the hypothesis that the ether is, in all respects, like a thin jelly, and I hardly think the difficulty diminished when its strains, as a rigid body, are required to be capable of producing permanent electrical forces. There are, of course, many ways in which matter may move through the ether besides by displacing it; as, for instance, in the way in which a volume of liquid water might pass through ice, namely, by dissolving in front, and by freezing as fast behind, and such hypotheses do not require any limit to be assigned to the rigidity of the ether. In all these cases it is, of course, evident, that when it is once shown that the energy of the medium depends on quantities which obey the laws of Maxwell's electric and magnetic induction and displacements, it

follows that the forces on the places that represent the electrified and magnetized bodies must be the known electric and magnetic attractions and repulsions; and one great difficulty in framing hypotheses as to the connexion of the ether and matter is in explaining how the matter moves through the ether.

A very beautiful theory of matter has been founded by Sir William Thomson upon Helmholtz's theory of vortex motion in a perfect liquid, and there seems no doubt that the simplest theory as to the constitution of the ether is that it is a perfect liquid. It seems unlikely, from Professor J. J. Thomson's investigations of the properties of vortex atoms, that the simple hypothesis that an atom is a mere vortex ring in a liquid otherwise at rest is a sufficient hypothesis, and it seems almost impossible to explain electric and magnetic phenomena without some further hypothesis. The hypothesis that the ether is like a thin jelly in no way *explains* this property, as it is the possession of properties analogous to rigidity that requires explanation. Now, it seems certain that the only way in which a perfect liquid can become everywhere endowed with properties analogous to rigidity is by being everywhere in motion. The most general supposition of this kind would be, that it was what Sir William Thomson has called a vortex-sponge, *i.e.* everywhere endowed with vortex motion, but with this motion so mixed up as to have within any sensible volume an equal amount of vortex motion in all directions. There are many ways in which this supposition seems to be in accordance with what we know of the properties of the ether. One would expect that electrostatic forces would be due to the irrotational parts of the fluid, while the electromagnetic forces would be due to the rotational parts. There are a great many ways in which such a vortex-sponge could be polarized. If it consisted of vortex filaments they might have either a solenoidal or lamellar polarization; and if it consisted of vortex rings they might also have their motions polarized so as to move parallel to lines or planes which would correspond to solenoidal or lamellar polarizations. Besides this, the axes of the vortex filaments or rings themselves might be

bent, or their sections distorted, introducing thus two new sources of forces. It is thus visible that the proposed supposition introduces modes of producing force between different parts of the medium in a sufficient number of ways to explain, not only electric and magnetic forces, but cohesional and chemical forces. I have not made any supposition as to the nature of matter. The supposition that the ether is a vortex-sponge in a perfect liquid, does not diminish the number of possible hypotheses as to the constitution of matter: on the contrary, it very much increases the possible modes of action of matter. I know very little as to the mechanics of a vortex-sponge, but some points mentioned by Sir William Thomson, in a Paper read before the British Association at Swansea (Brit. Assoc. Rep., Swansea, p. 474), seem in favour of the hypothesis. In the first place, the energy of any polarized state of vortex motion is greater than that of an unpolarized state, so that if the motion of matter reduce the polarization, there will be forces tending to produce that motion. Now, in the case of two infinite planes separated by a region of polarized vortex motion of any kind, it seems evident that, as the forces due to a small vortex vary as a high power of the distance from it, the forces due to the polarization between the planes will depend on this polarization, and not on the distance apart of the planes. This is characteristic of forces varying inversely as the square of the distance from points spread over a plane. Sir William Thomson has also pointed out a case in which a polarized state of the vortex motion will be in equilibrium as long as it cannot produce motion of the boundary of the containing vessel, but in which, if it can expend its energy in causing motion of the boundary, its energy will be so expended. From this it seems likely that there are modes of polarization of a vortex-sponge which will be in equilibrium as far as the sponge is concerned, and whose energy can only be spent on producing motion of the boundaries. This is exactly the condition required in order to explain the equilibrium of the strained ether, which, itself in equilibrium, tends to move objects within it. If the

vortex-sponge be otherwise disturbed, it is obvious that the polarized state will become distributed through the sponge, and this should be analogous to the propagation of light. It is evident that, inasmuch as we could neither create nor destroy vorticity, all we could do would be to polarize the motion, and that all such polarization would have this character of electric phenomena, that we could not produce one kind of electrification without producing somewhere an equal and opposite electrification.

I have not been able to work the theory out completely, but have not come across any fatal objection to it. There are a great many suppositions that can be made as to the nature of the polarization that represents electrostatic phenomena; and it may be of use, to anyone working at an hypothesis as to the nature of this polarization, to recollect that it is allowable to suppose a flow continuously in one direction between electrified bodies held apart by solids, for the electric forces are then balanced by stresses in the solid supports, and these stresses may be due to the flow back of whatever is supposed to be flowing out between the electrified bodies. I do not expect that any flow continuously in one direction does take place; but there is danger of such a hypothesis being summarily dismissed, because of its being supposed to lead to an accumulation in the body towards which the flow takes place.

In conclusion, I desire to reiterate that it seems that the only way in which to impart to a perfect liquid properties at all analogous to those possessed by the ether, is by supposing it full of motion.

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ON THE STRUCTURE OF MECHANICAL MODELS ILLUSTRATING SOME PROPERTIES OF THE ETHER

[From the *Philosophical Magazine*, June, 1885. Read at the Physical Society, March 28, 1885.]

THE elements of which the model is constructed consist of pairs of wheels so geared together that when one of them rotates it causes the second to rotate in the same direction. The simplest way of effecting this is to connect them by a band, and this is sufficient for a one-dimensional model. Such a model may be constructed by fixing a number of wheels with their axes parallel and at right angles to a plane, and connecting each wheel with its neighbours by elastic bands. This represents a non-conducting region of the ether. A perfectly conducting region is one in which there are no bands, and a partially conducting region would be represented by the bands slipping more or less. A short description of how electrostatic, electrokinetic, and luminiferous phenomena are illustrated by such a one-dimensional model will be clearer than the corresponding description of the tri-dimensional model, the structure of which is the special purpose of this paper.

As an illustration of an electrostatic phenomenon, consider two conducting regions separated by a non-conducting region everywhere except along one line where the bands are removed. If anywhere in this line a rotation in opposite directions be communicated to the wheels that abut on it, then all the wheels in the non-conducting region will be turned more or less. If anywhere two neighbouring wheels turn equally, there is no straining of the band connecting them; but if one turn more than the other, the connecting band is strained and one side

becomes tight and the other loose. Now it will be found in the case considered that all the bands are strained, and that all the tight sides are turned towards one conductor and all the loose sides towards the other. This represents the charging of the two conductors in opposite ways. The strain of the bands in any element of the medium represents the polarization of the element, and the line joining the tight and loose sides is the direction of polarization or of electric displacement. The energy of the system is in the form of this straining of the bands, which produces stresses tending to restore the unstrained condition. With a given strain at their respective surfaces, there would be more elements involved and more energy in the medium when the conductors are far apart than when near, showing that if we could represent in any way the fact that conductors can move through the ether there would be forces tending to produce this motion, or, in other words, there would be attraction between these oppositely electrified bodies. As, however, the model does not illustrate the connexion between matter and ether, neither this nor magnetic attractions are represented, nor have electromotive forces such as exist in cells been represented. If the forces that have been supposed to turn the wheels along the conducting line connecting the two conducting regions cease to act, the state of strain will disappear, and what represents an electric discharge along this line will take place. All along this line, during the discharge, the wheels at opposite sides will be rotating in opposite directions; so that this is what represents an electric current at any point. It is the same as an electric displacement; and in a non-conductor such opposite rotation is resisted by the stresses in the band, but in a conductor it may take place to any extent. During the discharge the whole of the non-conducting region is full of rotating wheels, and their axes of rotation are at right angles to the direction of discharge. Their velocity of rotation evidently represents the magnetic force accompanying the discharge, and the momentum of the wheels represents the electrokinetic momentum of the current, *i.e.* its self-induction. This is further illustrated by this, that if the

frictional resistance be small enough, this momentum will carry the wheels beyond their positions of equilibrium, and there will result an oscillating discharge, such as occurs when an electric condenser is discharged through a sufficiently small resistance. If we suppose a certain amount of frictional resistance at any point along the line of discharge, we may see that the energy expended on friction is conveyed to the place by the bands in the surrounding non-conductor and comes in at the side of the conductor, in accordance with Prof. Poynting's theorem as to the direction of the flow of energy in an electrodynamic system.

The mutual induction of two circuits may also be illustrated by the model. Sufficient has been explained, however, to show how electrostatic and electrokinetic phenomena are represented on the model. If a sudden movement of rotation be communicated to any set of wheels, it is evident that inertia will prevent their neighbours being instantaneously turned, while the connecting bands will be strained. Rotation will, however, be communicated to the neighbouring wheels, and from them to their neighbours, by a process which is a species of wave-propagation. If we consider the nature of the disturbance which is thus propagated, we see that it consists in a rotation whose axis is at right angles to the direction of propagation, and of a polarization of the bands which is at right angles both to the axis of rotation and to the direction of the wave-propagation. These are respectively a magnetic and an electric displacement, which are at right angles to one another and to the direction of wave-propagation. This is exactly in accordance with Maxwell's electromagnetic theory of light-propagation. It is thus seen how the same model that can represent electrostatic and electromagnetic phenomena also illustrates luminiferous phenomena by its small oscillations.

If we try to produce a tri-dimensional model by means of wheels geared together by bands, we are met by the following difficulty. The energy of the model we have been considering may be represented in the following way:—Let ζ represent the angular rotation of any wheel from a given position; then the

kinetic energy of an element will evidently be proportional to $\dot{\zeta}^2$, while the potential energy of an element, depending as it does on the difference of rotation of neighbouring wheels, will be proportional to $\left(\frac{d\zeta}{dx}\right)^2 + \left(\frac{d\zeta}{dy}\right)^2$. If we were to build up a tri-dimensional model by simply putting together three such systems of wheels in three rectangular planes, the kinetic energy of an element of the model would be proportional to

$$\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2,$$

but its potential energy would be simply proportional to

$$\left|\frac{d\zeta}{dx}\right|^2 + \left|\frac{d\zeta}{dy}\right|^2 + \left|\frac{d\eta}{dz}\right|^2 + \left|\frac{d\eta}{dx}\right|^2 + \left|\frac{d\xi}{dy}\right|^2 + \left|\frac{d\xi}{dz}\right|^2,$$

instead of

$$\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right)^2 + \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right)^2 + \left(\frac{d\eta}{dx} - \frac{d\xi}{dy}\right)^2,$$

which is what it should be if it is to represent the ether.

The electrostatic and electrokinetic energies of an element of the ether may be expressed in this form by assuming ξ, η, and ζ such that their velocities $\dot{\xi}$, $\dot{\eta}$, and $\dot{\zeta}$ are α, β, and γ, the components of the magnetic force at the element. Now simple band-gearing will not enable us to arrange that no straining shall result, when, for instance, $\frac{d\eta}{dx} = \frac{d\xi}{dy}$, but this may be accomplished by the following arrangement:—Each element of the ether is to be represented by a cube, on each edge of which there is a paddle-wheel. Thus on any face of the cube there will be four paddle-wheels. Now, if any opposite pair of paddle-wheels on a face rotate by different amounts, they will tend to pump any liquid in which the whole element is immersed into or out of the cube, and if the sides of the cube be elastic there will be a stress which will tend to stop this differential rotation of the wheels. If, however, the other pair also rotate by different amounts they may undo what the first pair do; and thus the stress will depend on the difference between the differential rotations of these opposite pairs of wheels, *i. e.* on $\frac{d\eta}{dx} - \frac{d\xi}{dy}$.

In order that these four wheels may not similarly work with any other wheel in the cube, it is necessary to place diaphragms, cutting the cube into six cells, each a pyramid standing on a face of the cube. These must be so made that liquid may not be able to pass from one cell to another through the diaphragm, nor beside the paddle-wheels. In order actually to effect this, the floats on the paddle-wheels would have to be drawn down while passing the diaphragm (of course these mechanical details could hardly be carried out so as to work with sufficiently light friction for the working of any actual model to approximate sufficiently closely to that of the ether for it to be worth while attempting to construct it). The faces of the cube should be filled up with diaphragms past which the paddle-wheels should pump liquid, and whose elasticity should be the means of storing electrostatic energy in the medium.

It may be worth while pointing out some of the results of having shown that the energy of a disturbance of this medium would be represented by the same equations as those Maxwell has shown to hold for the ether. The most complicated results follow from supposing the faces of the cubes, of which the medium is constructed, to have different elasticities. Such a structure represents a crystalline medium. Its vibrations would be propagated according to the laws of propagation of light in crystalline media. The wave-surface would be Fresnel's wave-surface, and it would exhibit conical refraction. If the cubes were twisted, the structure would be like that of quartz or other substances that rotate the plane of polarization of light.

In order to represent the rotation of the plane of polarization of light by magnetism, it would be necessary to introduce some mechanism connecting this ether with matter. That this would be required is evident from the consideration that no amount of inherent rotation of these wheels would alter the plane of polarization of a vibration transmitted by them. Now, although I am not prepared to suggest any actual method of connexion, it may be worth while pointing out two different ways in which it may work. In the first place the matter may be in rotation when subject

to magnetic force; and it may be connected with the ether in such a way that the direction of its axis and rotation is altered by the vibrations passing in the ether. It would then react on the ether in such a way as to rotate the plane of polarization of the wave. In the second place the matter might only act as a link connecting ether elements rotating in rectangular directions in such a way that the rotation of one element altered the axis of rotation of a rectangular element. The reaction of this latter on the former would then rotate the plane of polarization of a wave propagated parallel to the latter. The first of these connexions is that in accordance with Maxwell's theory as to the connexion between the ether and matter that he introduces in his "Electricity and Magnetism," in order to explain the rotation of the plane of polarization of light by magnetism; while the second is the one that I supposed in my paper "On the Electromagnetic Theory of the Reflection and Refraction of Light" (R. S. Trans. 1880).*

I need hardly say, in conclusion, that I do not in the least intend to convey the impression that the actual structure of the ether is a bit like what I have described. What physicists ought to look for is such a mode of motion in space as will confer upon it the properties required in order that it may exhibit electromagnetic phenomena. Such a mode of motion would be a real explanation of these phenomena. I have only given a description of them.

I think, however, that it is worth while considering these models, because in them the disturbance which represents light is not the same as the vibrations of an elastic jelly, for what represents an electric displacement is a change of structure of an element, and not a displacement of the element; and it seems almost certain that, notwithstanding the very high authority which seems to support the view that the ether is *like* an elastic jelly, nevertheless its vibrations are much more of the nature of alterations in structure than of displacements.

[* These are, in fact, the two theories referred to in footnote, p. 66 *supra*.]

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[FOUNDATIONS OF PHYSICAL THEORY: FUNCTION OF MODELS]

[Hitherto unpublished: written not later than 1888.]

ELECTRICAL and magnetic attractions and repulsions are familiar to most people. They know that amber and sealing-wax when rubbed attract small pieces of paper, and that the Earth causes magnetic needles to point more or less north and south. The first notion that struck investigators of these facts was that the amber and the Earth "possessed the property of attracting" certain objects at a distance from them. This certainly expressed all the facts as then known and expressed them simply. Such actions at a distance were easily expressed in mathematical symbols, and their laws were thus investigated and the whole language of the sciences was framed so as to fit in with this view of the phenomena. That these actions can be expressed in terms of symbols that presupposed an action at a distance does not, however, in any way prove that the action is not by means of some intervening medium. Take for instance a simple mechanical illustration. Suppose that you pull a body along by means of a string. This action may be expressed by symbols that only represent that the distance between you and the body is invariable, and that do not in any way involve the existence of a string, or indeed of any medium that transmits the action. It is only when you wish to take account of actions that take some time to be transferred from you to the body, it is only then that it is necessary mathematically to introduce symbols expressly referring to a medium. We may gather from this how it was absolutely necessary in the case of the action of light to

introduce the notion of something existing between the Sun, for instance, and the Earth, while it was not necessary, as far as was known, to make a similar assumption in regard of electrical actions. In the case of light it was known that it took time to be transmitted, while in the case of electrical actions they were, as far as was known, instantaneous, and could consequently be easily represented as taking place by an action at a distance. Although there are no experiments that prove certainly that either electric or magnetic actions are not instantaneous, nevertheless it is well worth while investigating how they may be due to a medium, for if they can be shown to be explicable by a medium possessing the same properties as are required in order to explain the transmission of light, the hypothesis that they are due to a medium will at once assume great importance, as these actions will then be explained as due to a known cause.

Before describing, in general terms, the sort of way in which it has been shown that electric and magnetic phenomena may be due to a medium, it may be well to call attention to how very little we know of the constitution of the media we use every day for transmitting force. Take the case of a beam supporting a weight. Nobody knows what the structure of its molecules is, by means of which they are able to resist compressive and shearing stress. But nobody on this account doubts the usefulness of considering and calculating these stresses. Similarly, although nobody has been able to frame any satisfactory hypotheses as to the constitution of the medium that causes electric and magnetic and luminiferous actions, nevertheless it is obviously very important to be able to calculate the stresses that it must be subject to at its various points in order that it may cause these actions. In the case of a clock or watch spring nobody knows what is the exact structure of its molecules by means of which it is able to act as a reservoir of energy, which we put into it when we wind up the clock or watch, and which is gradually expended in keeping the works going. The fact that we do not know this does not in the least

diminish from the importance of our knowing that there is a spring, and that the action is not a pure action at a distance between the axle that we turn with the key and the barrel that turns the wheels. Similarly it is of importance to know where in the medium the energy of an electrified system is stored, although we may not be able to state what the exact structure of the medium must be in order that it may be capable of acting as a reservoir of energy.

The calculation of electric and magnetic actions is a subject upon which mathematicians have expended some of their highest abilities, and the simplicity of the general laws of these actions lends itself to mathematical calculation to no ordinary degree. Mathematically it was simpler to represent the actions as actions at a distance, and not to introduce symbols representing the existence of a medium intervening between the bodies concerned. Hence it was that the whole of the language of the science was framed upon this hypothesis, and it seems likely that the counter-hypothesis of action through a medium might have had to wait as long as the undulatory theory of optics did before it received due consideration, only for the investigations of one who never received a mathematical training. To one of less ability than Faraday this would have placed him at an immense disadvantage in comparison with trained investigators. In his case, however, his wonderful power of reasoning from things without the use of symbols not only placed him in the forefront of scientific investigators, but enabled him to see into the method of working of the phenomena in a way that escaped mathematicians who reasoned by the use of symbols. To Faraday is due the credit of having seriously called attention to the notion that electric and magnetic actions were due to a medium pervading the space between the acting bodies. This hypothesis of Faraday's was expressed in mathematical symbols by Clerk Maxwell, who then made the great discovery that a medium which possessed the properties required in order to explain electric and magnetic actions would transmit vibrations like light.

There is a term describing the condition of the medium that it is necessary to explain by examples which, although they may not be *like* the condition of the medium, are so far *analogous* to it, that we may in many respects reason safely from one to the other. This term is "polarization." A thing is said to be polarized when it possesses different properties in different directions. Thus a magnet is said to be polarized because it possesses different properties at its ends. A ray of light is said to be polarized when it possesses different properties at different sides. A mechanical illustration may be imagined by considering two wheels, on parallel axes, connected by an indiarubber band passing over both. As long as the two turn equal amounts, the two connecting parts of the band will be equally tight. If, however, one wheel turn more than the other, two parts of the band will no longer be equally tight, and the system may be said to be polarized, for its two sides are no longer the same, for one is tight and the other loose. The direction of the polarization would, in this case, be the line joining the tight and loose parts of the band, and at right angles to the line joining the centres of the wheels. In this case the polarized system is a reservoir of energy, and there are forces tending to cause rotation of the wheels. It is of great importance to understand what sort of thing is meant by "polarization," because the whole of Faraday's theory is founded on supposing the medium to consist of elements which are capable of being polarized, and it is supposed that a certain amount of energy is stored in the polarized elements.

Without introducing considerable complications, it is not very easy to explain how such a supposition as this can explain electric and magnetic forces; but some general considerations will show how such a supposition can be made to explain the existence of some forces between bodies. Consider the simple case of two bodies in a medium in which energy in any form is stored, and suppose that the amount of this energy depends on the distance between the bodies in such a way that the farther the bodies are apart the more energy there will be in the

medium. Under these circumstances there must be some forces tending to draw the bodies together; for, by hypothesis, in separating them, work must be done in increasing the energy in the medium, and as work is done in separating them there must be a force drawing them together. Now there are two ways in which energy may exist in such an element as has been described, consisting of two wheels connected by an elastic band. It may exist as stored in the elastic band when the element is polarized, or it may exist as energy of rotation of the wheels. Now Clerk Maxwell's theory is that electric actions are due to polarization of the elements of the medium, and that magnetic actions are due to its elements being reservoirs of kinetic energy. It is, of course, quite possible that all energy is really kinetic, but until some workable hypothesis as to the structure of the medium has been discovered that will explain its property of storing energy by being polarized, and which explains this property by storing kinetic energy, until some such hypothesis has been discovered it is well to distinguish the energy that is stored by the polarization of the elements of the medium from what is stored in the form of motion within those elements.

Before proceeding to explain how the electric and magnetic conditions of the ether may be illustrated by a model, it may be well to say a few words upon the general question of physical analogues. There is some danger in using physical analogues as assistants to our reasoning. There is no doubt but that a concrete mechanism that we can distinctly picture the working of is enormously easier to reason about than one of whose structure we know nothing, but only know general laws of its action. The danger is that we may be satisfied with an analogy, and mistake it for a likeness. For instance, the laws of projectiles and those of light are in a great many respects the same, so that an analogy between the propagation of light and of a projectile can be drawn, and it was drawn and was supposed to be a likeness for more than a hundred years. From the time of Newton until far into the present century, light was supposed

by a large number of scientific men to be due to projectiles emitted by luminous bodies. All reasoning about nature is, however, in part necessarily reasoning from analogy. Whenever we express a general law about things whose intimate structure we do not know, our language may be, for aught we know, at least partly of the nature of analogy. When we describe a current of electricity as running, we are almost certainly using a mere analogy. It is usual to speak of chemical bonds; and many people think of light vibrations as simple to-and-fro motions of the elements of an elastic jelly: in both these cases we ought to be careful to recollect that "bond" and "vibration" are capable of a much wider meaning than their usual simple ones, and that the wider meaning is possible, because some of the laws of a great variety of different things are the same. As a very simple example of this, the laws of the addition of money, length, weight, etc., are all the same: 12 pence and 3 pence is 15 pence, and 12 yards and 3 yards is 15 yards. In a similar way the laws of conduction of heat and of diffusion of gases are in many respects the same. Notwithstanding the danger of our mistaking analogies for likenesses there is a great advantage in studying analogues. It has been objected to the quaternion analysis that it does not analyse enough, but leaves the quantities we deal with nearly as complicated as to their laws as the phenomena they represent. This is certainly in some respects a disadvantage. It might similarly be objected to a mechanical model of the ether that it was as complicated as the ether itself, or possibly much more so, and could consequently be of but little use in disentangling its laws. In both cases the same answer is possible. By analysing too much we lose a comprehensive view of the subject. Our minds are overwhelmed with the details, and we fail to grasp the action as a whole. In the case of a mechanical model of the ether, we have before us a structure which we may easily conceive, and with the method of whose working we are familiar; and so we can reason about it and discover what it should do without being troubled at every turn to realise our analysis, for

the realisation is so easy and familiar that it gives our minds no trouble. As an example, it is worth observing that if the forms of energy were as familiar a conception as eggs and money, people would find it as easy to reason about its transformation as they are about the number of eggs the old woman brought to the market and sold at one dozen at 3 a penny and so forth. It is because at every turn people lose the thread of the argument by the difficulty of realising what they are dealing with that they find it difficult at first to reason consistently about a new subject. It is on account of this that it is worth while studying analogies between things with which we are not familiar and those with which we are. It is for a similar reason that having made ourselves familiar with the laws of algebra we can so readily apply it to geometrical analysis. A model of the ether is like a complicated algebraic formula that can be interpreted in terms of a known analogy so as to represent the laws of a surface; it is not like the surface, but we may deduce the laws of one from the other if we attend to the laws of the analogy. We must not press analogies too far. To suppose that the ether is at all *like* the model I am about to describe would be as bad a mistake as to suppose a sphere at all like $x^2 + y^2 + z^2 = r^2$, and to think that it must in consequence be made of paper and ink.....

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SIR W. THOMSON AND MAXWELL'S ELECTROMAGNETIC THEORY OF LIGHT

[From *Nature*, May 7, 1885.]

SHORTLY after writing my former letter I saw a copy of the verbatim report of Sir William Thomson's lectures in Baltimore, and would have written to you to that effect and to apologise to Mr. Forbes for having doubted the accuracy of what I thought was his report, only that I met him in London about that time, and he then desired me not to do so. Sir William Thomson has now himself stated that the passage is correctly quoted, and I can only regret that he has expressed himself in the way he did.

I certainly think that anybody reading the passage would imagine that the velocity of propagation of electromagnetic disturbances upon Maxwell's electromagnetic theory of light, which he showed to be the same as the velocity of propagation of light, and to be a true velocity of wave-propagation—any one, I say, would suppose that this was the same thing as that Sir William Thomson calculated in the year 1854.

Sir William Thomson certainly says, "that is a very different case," but the rest of this sentence is rather ambiguous as to what the "it" after "putting" refers to, and I am afraid that many people will imagine that, in Sir William Thomson's opinion, Maxwell has made some unjustifiable assumption. I believe, however, that all he thinks is that Maxwell has not made a satisfactorily definite thing of the so-called electromagnetic theory of light.

In Sir William Thomson's article in Nichol's "Cyclopædia" he puts the matter very clearly indeed. He says:—"The law

of this phenomenon [transmission of electric signals] is identical with that which Fourier . . . found as the law of propagation of summer heat and winter cold to different parts of the Earth," *i. e.* it obeys the laws of a diffusion and not of a wave-propagation; and again:—"Now it is obvious from these results [experimental results] that the supposed velocity of transmission of electric signals is not a definite constant like that of light": and afterwards he says that, when an initial current is started, the potential rises simultaneously at all points, and that the apparent velocity would depend on the delicacy of our instruments. All these obviously distinguish between the propagation of a variable current in a conductor and a true wave-propagation.

He has also clearly pointed out a direction in which to look for a true wave-propagation. It will make his position clearer, and also Maxwell's, to use his analogy between water in an elastic tube and a conductor of electricity. I will suppose the water contained in a tube bored out of a very large lump of indiarubber. He enumerates three electric qualities concerned, and their hydrodynamic analogues:—(1) "Charge" or electrical accumulation in a conductor subjected in any way to the process of electrification. (2) "Electromagnetic induction" or electromotive force excited in a conductor by variations of electric current. (3) Resistance to conduction through a solid. The hydrodynamic analogues are:—(1) Accumulation of a greater or less quantity of water in any part of the canal or tube. (2) Inertia of the water. (3) Viscosity or fluid friction. He explains that a true wave-propagation arises from the compressibility of the water, combined with its inertia; and that if the tube be elastic, like indiarubber, there would also arise a wave-propagation. "Accordingly," he says, "a definite velocity of propagation of electric impulses, depending on the inertia and the capacity for charge, is to be looked for, as has been done in a first article, published by Kirchhoff, on the subject."

Now, in all this discussion Sir William Thomson omits to mention the only thing that is at all analogous to Maxwell's

propagation of wave disturbances in non-conductors, and it arises from his considering the water as contained in a tube like ordinary indiarubber tubes, instead of in a tube bored in an indefinitely large lump of indiarubber. If we consider this case it is evident that one of the conditions to be considered is the propagation of waves in this lump of indiarubber. In Sir William Thomson's tube there would of course be a velocity of wave-propagation in the indiarubber, but that is a very different matter from the propagation of disturbances away from the neighbourhood of the tube by which energy would be carried away from it. To do this Sir William Thomson should have included the propagation of sound in the air or whatever he supposed surrounding the outside of his tube. Without including this, he was not including anything a bit analogous to Maxwell's electromagnetic theory of light. In Sir William Thomson's tube the whole state of affairs at any time could be expressed in terms of variables that represented bodies near the tube, while in the other case it would be absolutely necessary to introduce variables representing every part of the indiarubber which I have supposed of indefinite extent. This is just the difference between Sir William Thomson's and Maxwell's views. According to Maxwell's view there is a great deal more going on outside the conductor than inside it, and it is evident that the inertia of the water is a very bad analogue to electromagnetic induction, for this latter depends essentially upon the form of the circuit, and not only upon its section and length. Maxwell has shown that light may be a wave-propagation of what are on his theory *analogous* to, though probably utterly *unlike*, the distorsional waves propagated in the indiarubber, and has shown that a medium which would only transmit disturbances analogous to these would explain electric and magnetic phenomena. It is to be remembered that Maxwell's theory gets rid of all action at a distance, and that the only *experimentum crucis* between theories of action at a distance and of action through a medium is that in this latter case the energy may be propagated in time through the medium, while in the former it cannot.

I cannot conclude without protesting strongly against Sir William Thomson's speaking of the ether as *like* a jelly. It is in some respects *analogous* to one, but we certainly know a great deal too little about it to say that it is *like* one. May be Maxwell's conceptions as to its structure are not very definite, but neither are anybody's as to the actual structure of a jelly, and there is no real difficulty in supposing a medium whose condition is represented by symbols that obey the laws that Maxwell has shown should be the laws of symbols representing the condition of a medium that would explain electric and magnetic phenomena. It seems very unlikely that any jelly is at all *like* the ether that Maxwell supposes. It seems much more likely that what he called "electric displacements" are changes in structure of the elements of the ether, and not actual displacements of the elements. He guards against this being supposed a necessary part of his theory when he defines polarization in terms that certainly require a change of structure rather than a change of position, so that I think the word "displacement" was unfortunately chosen. I also think that Sir William Thomson, notwithstanding his guarded statements on the subject, is lending his overwhelming authority to a view of the ether which is not justified by our present knowledge, and which may lead to the same unfortunate results in delaying the progress of science as arose from Sir Isaac Newton's equally guarded advocacy of the corpuscular theory of optics.

37

NOTE ON THE SPECIFIC HEAT OF THE ETHER

[From the *Scientific Proceedings of the Royal Dublin Society.* Read May 18, 1885.]

IT is not quite accurate to describe the quality of the ether that is here called its specific heat by that name. The energy of the ether per unit volume is not in exactly the same form as heat-energy in matter. It is in the form of vibrations transmitted continually through it, and is more nearly analogous to sound-vibrations in matter. As this energy of the ether is, however, intimately connected with the temperature of the matter that is in it; and as its amount depends on that temperature, it is quite intelligible to speak of the temperature of a volume of ether, meaning that it is on all sides in temperature equilibrium with surrounding matter: on these accounts it is intelligible, and, with proper restrictions as to its meaning, quite right to speak of the specific heat of the ether. There is no danger of the words being mistaken as referring to anything else than what they are intended to describe. It must, however, be clearly kept in view that the energy per unit volume of the ether is not exactly like that of matter. We can also only calculate the energy per unit volume, for we don't know whether any perfectly intelligible meaning can be attached to the mass of the ether.*

The problem to be solved in determining the specific heat of the ether is, What is the increase in the quantity of energy per

[* See footnote, p. 121.]

unit volume of the ether that corresponds to its temperature being raised one degree? If we consider the temperature equilibrium of a surface of lampblack, which is assumed to be approximately a perfect absorber and radiator, when it is one degree hotter than its surroundings, it is seen that it is radiating energy outwards at a certain rate and receiving it at a certain less rate. Now, the energy per cubic centimetre of the ether considered as transmitting radiations outwards is greater than its energy considered as transmitting radiations inwards, and it is this difference that causes the transference of energy at a certain rate away from the lampblack. If we could calculate the whole energy of either of these radiations, it would tell us the whole energy per cubic centimetre of the ether considered as a medium transmitting radiations in a particular direction, and six times this amount would evidently be the total heat per cubic centimetre of the ether at this temperature.* This very calculation has been performed approximately for the solar radiations, where we can approximately measure the total radiation in a particular direction; but as we only know very roughly, indeed, what is the temperature of the surface of the Sun, we can only calculate very roughly what is the total heat of the ether per cubic centimetre at that temperature. As in addition we don't know the law of cooling for any sufficiently large range to apply it safely to the Sun, we could only very roughly, indeed, calculate the total heat of the ether per cubic centimetre at temperatures with which we are in the habit of dealing. We can, however, from experiments on the rate of cooling of lampblack, calculate pretty accurately the difference between the energies per cubic centimetre of the ether at

[* A perfect absorber radiates equally in all directions; thus the emission from it, per unit time, in directions within a cone of infinitesimal solid angle $\delta\omega$ is $I\delta\omega$ multiplied by the area of radiating surface foreshortened along the axis of the cone. The volume-density of radiant energy, travelling in all directions within a thin layer of ether in front of the radiator is therefore, per unit surface, $2V^{-1}\int I d\omega$, or $4\pi I/V$; while the radiation emitted per unit time is $\int I \cos\theta d\omega$, or πI. This makes dq/dt equal to $\frac{1}{4}$ of cV; the fraction $\frac{1}{4}$, assumed in the text, appears to have been suggested by a unit cube radiating directly outward from its six faces.]

slightly different temperatures, and this tells us the specific heat of the ether at this temperature. If $\frac{dq}{dt}$ be the quantity of heat per second lost by each square centimetre of surface of lampblack when its temperature is 1° C. hotter than its surroundings at the temperature of 0° C., then this energy is distributed over V cubic centimetres of ether where V is the velocity of light in centimetres, and consequently $\frac{dq}{dt} = \frac{cV}{6}$ is the equation by which to calculate c, the specific heat of the ether. The 6 comes in owing to this being the energy required to raise the temperature of the ether by radiations going in only one direction. From several sources which, however, only approximately agree with one another, I gather that $\frac{dq}{dt} = \cdot 0026$. The great difficulty in determining this is to distinguish between the heat carried off by the gas present, and that lost by radiation alone. As $V = 3 \times 10^{10}$, we see that $c = 5 \cdot 2 \times 10^{-13}$. Of course, as was to be anticipated, this is a very small quantity, and is very small even compared with the specific heat per cubic centimetre of the gas in a good vacuum. The specific heat per cubic centimetre of an air vacuum of one-millionth of an atmosphere, such as we can probably attain, is about 7×10^{-10}, so that the energy required to heat up such a vacuum would be practically all used in heating the matter present, only the one-thousandth part being required to increase the vibrations of the ether present.

If we apply Dulong and Petit's law [of cooling] to calculate the specific heat of the ether at different temperatures, we obtain the following results. We have obtained the equation that

$$\frac{dq}{dt} = \frac{cV}{6}$$

expresses the relation connecting $\frac{dq}{dt}$, the rate of cooling with c, the specific heat of the ether. Now, according to Dulong and Petit's law

$$q = k \cdot (a^t - a^t_0),$$

where q is the quantity of heat per second lost by radiation, when one body is at t and its surroundings at t_0. They also calculated that for all bodies $a = 1{\cdot}0077$. As I am assuming the theory of exchanges, I may as well at once assume that the total quantity of heat lost per second is $Q = ka^t$, and that there is $Q_0 = ka^{t_0}$ radiated in the opposite direction, so that what I have called the total heat of the ether is

$$H = \frac{6ka^t}{V}.$$

To calculate c we can use the result I have just used, and say

$$\frac{dq}{dt} = \frac{cV}{6} = ka^t . \log_e a,$$

and we thus get

$$k = {\cdot}35;$$

so that, approximately, $Q = {\cdot}35 \times (1{\cdot}0077)^t$. From this we can calculate the total heat per cubic centimetre of the ether at different temperatures, and get

$$H = 7 \times 10^{-11} \times (1{\cdot}0077)^t,$$

and the specific heat per cubic centimetre

$$c = 5{\cdot}2 \times 10^{-13} \times (1{\cdot}0077)^t.$$

Even at the estimated temperature of the Sun at 3000° C. this would be still only

$$c = 5{\cdot}2 \times 10^{-3};$$

but it is evident that if the specific heat of the matter in a gas is at all nearly constant up to these temperatures, a very large part of the heat energy would be in the ether if the gas were rare. This is what one would expect in considering temperatures of spark discharges, and accounts for their very short duration.

It is quite easy to apply the same considerations to calculate these quantities upon other laws of cooling, but this whole subject is so uncertain it is hardly worth doing more than point out what is to be done.

38

ON SOME METHODS OF MEASURING THE DENSITIES OF GASES

[From the *Scientific Proceedings of the Royal Dublin Society.* Read June 15, 1885.]

THE following methods depend on determining the amount by which a body is buoyed up when immersed in the gas.

The first arrangement consists of a large thin glass ball sealed up and suspended by a bifilar suspension. The period of oscillation of such a ball round a vertical axis depends on the distance apart of the suspending fibres, on its moment of inertia, and on the weight of the ball. This latter depends on the density of the gas in which it is immersed, and by observing the time of oscillation of the ball it is possible to determine the density of the gas. In order to do this conveniently it is well to make observations of the period of oscillation in two gases of known densities, as for instance in air at the ordinary atmospheric pressure, and at a very much lower pressure obtained by exhausting by an air-pump the vessel in which the ball is suspended. As the relation connecting the number of oscillations per second (N) of the ball with the density (δ) of the gas in which it is immersed is of the form $N^2 = A + B\delta$, two observations serve to determine A and B, and then any other density can be determined in terms of N, the number of oscillations performed by the ball. From experiments I made I believe it is easy to arrange that the ball shall perform one hundred oscillations without their amplitude being either initially too large or finally too small; and if by making the

length of the suspending threads long (this is better than making their distance apart small, as this latter introduces a correction depending on their torsion), the period of oscillation be considerable, it is evidently possible to determine this period very accurately. It is better to determine the period of this oscillation round a vertical axis than its period of oscillation as a pendulum, because, without a very long suspension, this latter would be more rapid, and would besides be more damped by the gas present. If, however, means were employed for keeping up the oscillations, as, for instance, by attaching a small piece of iron to the ball, and attracting it when in the middle of its swing by means of a magnet, or by moving the point of suspension, or by other means, the period of oscillation could be determined to any desired degree of accuracy.

The complete theory of the experiment is, however, very complicated. The highest mathematics have been expended on calculating the effect of the gas present on the period of oscillation of a pendulum; and although the theory of a sphere performing oscillations round one of its diameters is enormously simpler, yet the way in which its period of oscillations depends on the viscosity of the gas is rather complicated,* and this would make it a rather complicated operation to calculate accurately the density of the gas from observations with this instrument. It might, however, be used for rough observations, and as the same series of observations by giving the logarithmic decrement

[* For these calculations see Lamb's "Hydrodynamics." The torsional oscillations of a sphere are treated on p. 568, where the retarding torque arising from viscosity is determined. Its first term alone affects the frequency N, when the retardation is small; the other term determines the rate of subsidence. The final result comes out to be

$$\frac{N^2}{N_0^2} = 1 - \frac{\delta}{\delta_0} - 5\frac{\delta}{\delta_0}\frac{1+\beta a}{1+2\beta a+2\beta^2 a^2},$$

where δ/δ_0 is the ratio of the densities of the gas and the sphere; $\beta = (N_0/4\pi\sigma)^{\frac{1}{2}}$, where σ is the kinematic coefficient of viscosity, which is of the order 10^{-1} c. g. s. for gases and is inversely as the density for each gas; and a is the radius of the sphere. Thus the simple formula in the text holds good only when βa is a small fraction, or when it is of order higher than $\frac{1}{2}$: in both these cases B/A is the same for all gases.]

of the amplitudes of oscillations would give a measure of the viscosity of the gas, it would be an interesting instrument to make observations with on a considerable series of gases and vapours.

The second instrument I constructed works upon the same principle as an old philosophical instrument called a "Baroscope." It was constructed for the purpose of demonstrating the amount by which bodies are buoyed up by the air in which they are immersed. A piece of cork or a glass bulb is balanced in a rough balance by a lead weight. If this be placed under the receiver of an air-pump, the lead weight no longer balances the cork or bulb, as it is no longer assisted by the presence of dense air. A delicate apparatus of this kind is evidently capable of exactly the same accuracy as the ordinary method of weighing the gas in the bulb, as in Dumas' method, for it is only weighing the gas displaced by the bulb instead of that in it. As, however, most delicate balances are large, I thought it might be well to construct an apparatus of this kind on a very small scale, so as to be able to deal with comparatively small volumes of a gas. I have, therefore, constructed one as follows:—I blew a thin glass bulb of about 1 cm. diameter on a fine quill tube, which I drew out to a thinner stem about 2 cm. long. To the end of this I attached a mirror such as is used for galvanometers, with its plane at right angles to the stem. At a point in the stem close to the bulb I attached two very small and sharp splinters of glass which projected away from the stem in such a way that the whole thing could be supported on the two sharp points of the glass splinters. By bending the stem judiciously, and attaching small pieces of glass to regulate the balance, it is comparatively easy to arrange that the whole thing shall balance on these two sharp points, and perform slow oscillations, proving that its centre of gravity is close below the line joining these points. The mirror should be vertical, and the stem nearly horizontal. If it be now supported by the two points resting in the concave side of a splinter of a fine glass tube, the whole apparatus can bear to be moved about freely without shaking

off its supports. If it be now placed so as to reflect a spot of light from the mirror to a vertical scale, or if an image of a vertical scale in its mirror be viewed through a telescope, it is possible to observe the position of the instrument very accurately. If the density of the air in which the apparatus is immersed be changed, its position of equilibrium alters, and the spot of light is reflected to a different point of the scale. I have constructed an instrument of this kind, in which the alterations of position on a scale about one metre off are as great as the alterations of the mercury barometer gauge of the air-pump, showing that it is possible to determine pressures with this instrument nearly as accurately as with a mercury barometer gauge. This is of course the same as determining the density of the air, for it is the density and not the pressure of the gas present that affects the position of the instrument. I have found that the most satisfactory way of using the instrument is to have some means, as by an attached Sprengel or other air-pump, by which to alter the pressure of the gas in which the instrument is immersed, until the spot of light or image of the scale is in some standard position, such as it takes up in air at a known temperature and pressure. Under these circumstances the density of the gas is the same as that of this air, and an observation of the temperature and pressure of the gas enables its density at any other temperature and pressure to be calculated. As the whole thing can be put inside a tube four centimetres long, and three in diameter, it could evidently be easily jacketed so as to keep it at a high temperature, and then be used to measure the densities of vapours, the only precaution required being that the temperature should be sufficiently high to prevent the gas from condensing as a liquid on the apparatus. A small correction might also be required for the expansion of the glass by heat. As the whole thing can be made of glass, for even the mirror need not be silvered, it is applicable to such corrosive gases as chlorine, bromine, and iodine.

It is evident that a large number of applications of the same general principle are possible: as, for instance, by weighing a

bulb immersed in a vapour, as in Mr. Joly's calorimeter, in an ordinary balance, or by using spring-balances to determine the weight of the bulb. A very pretty one might be made by an application of Messrs. Ayrton and Perry's shaving springs, and it might be made very delicate by reading the position of the bulb by reflection from a mirror attached to it. By suspending a thin balloon of collodion, distended by a heavy gas, in a gas, the density of the latter might be measured with a very delicate balance that would not bear the weight of a glass bulb.

39

ON THE LIMITS TO THE VELOCITY OF MOTION OF THE WORKING PARTS OF ENGINES

[From the *Scientific Proceedings of the Royal Dublin Society.* Read March 24, 1886.]

ENGINES are used for transforming one kind of energy into another.

Mechanical engines are of two great classes—those that transform potential or statical energy into work, and those that transform kinetic energy into work.

Slow-moving overshot waterwheels may be taken as types of the first class, and windmills as types of the second class. In all cases, it is of course possible by mechanical contrivances, such as levers, pulleys, wheels, etc., to obtain any velocity of moving parts; but the velocity I am calling attention to is the velocity of the parts that move with the working substance. Now, in the case of waterwheels it is evident that when the wheel turns so fast that the water in the buckets is descending as fast as it would fall freely, there can be no work being done by the water on the wheel, and so this limits the rate of working of the wheel. It is to be remarked that in the limiting case the efficiency is zero, while the power is zero when the efficiency is a maximum, *i. e.* when the wheel is turning most slowly, and that there is a rate of working intermediate between these for which the power is a maximum. In the case of windmills, when the sails turn so fast that the wind blows on unstopped, there is similarly no work being done, and, just as in the other case, this limits their velocity.

Heat engines are of a different class, as they are for the transformation of irregular into regular motion; but their mechanical, as distinct from their thermal, arrangements may be grouped as in the last case. Ordinary steam engines work by means of the energy in the steam doing work by pressing on a piston, and evidently this piston cannot move faster than the steam can follow it up. Professor Osborne Reynolds has, in the March number of the *Philosophical Magazine* this year, called attention to the way in which the velocity of flow of a gas into a vacuum is limited, and this limits the velocity of motion of the piston in an engine. He has, however, omitted to notice that there is a greater velocity than the velocity of sound with which a gas can move into a vacuum, namely, at the rate at which its particles are moving. This only comes into effect when the space is so small, compared with the free path, that we cannot deal with the molecules as making an indefinite number of encounters on their way across the vessel. In the case, for instance, of a piston in a vessel full of a gas moving suddenly from rest, with a velocity equal to that of the average velocity of the molecules of the gas, which is greater than the velocity of sound in the gas, it is also evident that all the molecules that were just on the point of striking the piston would follow it up, and that those that happened to be moving normally to it would keep following it up, and so would be diffusing into this vacuum at a greater rate than the velocity of sound in the gas. This leads to a diffusion velocity of energy in a vacuum small compared with the free path, quite different from the velocity of sound, and upon which evidently radiometer action depends. It is this that would ultimately limit the rate at which the piston could be moved by the gas. I have explained this at my lectures on the Theory of Steam Engines for some years back. Steam may also be used kinetically, as in Giffard's injector and Hero's engine; and in these cases velocity of motion is limited by the velocity of flow of the steam.

In the case of most of these engines that transform kinetic energy into work, it is to be remarked that when moving slowly

there is a very small power produced at the expense of a great expenditure. For example, in Hero's engine and engines of this type, if the steam runs out freely without moving the engine, there is certainly the maximum pressure tending to move the parts, but no power is produced, even though a great deal of steam is being employed. It is not the same with pressure engines, like ordinary steam engines. They may be worked slowly, and the power produced is proportional to the steam employed. The same distinction holds in the case of water engines working pistons, and turbines. In the case of the kinetic engine we must work rapidly if we are to get a good efficiency, for the efficiency vanishes at the slow limiting velocity. In the case of statical engines, the efficiency is a maximum when they are working at their slow limiting velocity, and vanishes when working at their quick limiting velocity. In the case of a perfect turbine, the efficiency is a maximum when going at its quick limiting velocity.* In the case of water engines there is evidently a limiting velocity also depending on the rate of propagation of energy by the water, *i. e.* its rate of propagating sound. Gas engines have similarly a limiting rate of working depending on the rate of explosion, *i. e.* of propagation of energy by the working substance.

Capillary engines and muscles have probably limits of rates of working analogous to those depending on the rates of diffusion of the molecules of the working substances at the working surfaces. We know that muscles like kinetic engines have a zero efficiency when working at their zero limit of velocity, and there is almost certainly a maximum limit to their rate of working. Capillary engines, like M. Lippmann's, are evidently limited by the rate of diffusion of the molecules at the capillary surfaces, *i. e.* of the superficial energy.

Electric engines have got analogous properties. There are the two classes—electrostatic engines, such as a reversed Holtz machine, and electrokinetic engines, such as ordinary magnetos and dynamos. The former can be worked as slowly as we please,

[* Cf. the recent steam turbines of Parsons and of Laval.]

without waste of energy; but the latter require to be worked at near their limiting velocity to have a good efficiency. A limiting velocity in the case of dynamos is well known, and is attained when the inverse electromotive force of the dynamo is equal to the driving electromotive force; but with a given electromotive force it does not seem at first sight as if there were any limit to the rate of working of a Holtz machine or any electrostatic engine.

If we, however, consider the electromagnetic action of moving electricity, it becomes evident that the forces between the different parts of an electrostatic engine must diminish as its velocity of motion increases, until its parts have a relative motion equal to the velocity of light, when there will be no more forces between them. If it move faster than this, it will become an electromagnetic engine, for the electromagnetic forces will become greater than the electrostatic. The way in which this acts is as follows:—Suppose a charged body, *e.g.* the carrier in any of the multiplier forms of any electrostatic engines, move near a conductor, it induces on this latter an electric charge which moves along with the moving carrier. I must neglect the resistance of the conductors, because it, being of the nature of friction in ordinary engines, limits the velocity in quite a different way from the ways I am considering. Now, if the carrier move with the velocity of light, it and its induced charges will have no action on one another, and so there will be no forces tending to move the carrier. Similarly, if a plate with a charge on it move parallel to a conducting plate, the moving electrification while its velocity is increasing induces a current in the conducting plate which is permanent, because the conducting plate is supposed to be a perfect conductor, and the electromagnetic action of these two, when the moving plate moves with the velocity of light, is equal and opposite to their electrostatic attraction. Thus it appears that the velocity of light is a limiting velocity to the rate of motion of these engines, just as the velocity of the particles of steam is a limit to the rate of motion of the piston in a steam engine.

There is the same limit to the rate of working of electromagnetic engines. Consider a very simple case. Suppose a wire sliding on two parallel rails with a magnetic force at right angles to their plane, and an electromotive force driving a current round the circuit. If the magnetic force be feeble enough there seems at first sight no limit to the ultimate velocity of motion of the wire. If we consider, however, what takes place when the electricity goes across from the rails to the moving wire, we see that the reason it goes across is because an electrification on the rails induces a charge on the moving wire, and these attract one another and combine, this action being kept going constantly by the fresh charges supplied by the battery. Now if the wire move with the velocity of light, there will be no longer any action between these charges, and so the wire will act practically as a non-conductor. A conductor moving with the velocity of light acts in other respects as a non-conductor, for it is evident that we can have any desired distribution of electricity in it or on it without any tendency for it to change.* It would be more correct to describe it as a region in which the electrostatic inductive capacity was infinite, and where, consequently, a given charge produced no force in its neighbourhood. There is another way of looking at this question, and one that leads to another view of the reason for this limiting velocity. It depends on the theory put forward by Professor Poynting that the energy given out at any point in an electric circuit is transferred there through the ether, and as energy is transferred through the ether with the velocity of light, it cannot keep up with a moving body that moves with a greater velocity than this. This completes the very remarkable analogy between the way in which the rate of motion of a piston by a gas is limited by the rate of propagation of energy in the gas, and the rate of motion of electric engines is limited by the rate of propagation of energy in the ether.

[* See, however, Introduction.]

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ON THE TEMPERATURE AT VARIOUS DEPTHS IN LOUGH DERG AFTER SUNNY WEATHER

[From the *Scientific Proceedings of the Royal Dublin Society.* Read April 21, 1886.]

THE measurements upon which this Paper is founded were made by me in the month of July, 1876, and I would have hardly thought them worth recording only that I have lately seen it noticed as a new fact that the isothermal surfaces in the Lake of Geneva are not level surfaces; and that this was so in Lough Derg was one of the special features I remarked in my observations of nearly ten years ago.

I made experiments with a maximum and minimum thermometer, attached to a sounding-line, and the differences of temperature observed were so great that there could be no doubt, even with rough experiments.

The observations were made after a long continuance of hot, sunny weather, during which the day temperatures ranged from 73° F. to 75° F., and the night temperatures from 55° F. to 65° F.

The temperature of the surface of the lake rose rapidly during sunshine, at a rate of nearly a degree per hour. In the deep water the temperature of the surface did not rise so fast as in the shallow water. About 3.30 in the day the temperature of the surface water in the deep parts was 71° F., and in the shallows 75° F. From a calculation of the amount of heat that enters the water, it seems that only about $\frac{1}{50}$th, or less, was used in heating it, the rest being probably spent in evaporation.

During the evening the temperature of the surface fell slowly, until in the morning it was uniform to a depth of about 5 yards, this being the depth, apparently, that the convexion currents during the night reached. This temperature was, on the night I observed it, eleven degrees above the night temperature of a thermometer exposed on grass. In the shallow water the temperature fell more rapidly until it was about two degrees colder than the surface water in deep parts, and nearly the same as that of the water at the bottom of the deep parts of the lake. It thus appears that the cold water supply for the bottom of the lake may be kept up by the cold night water from the shallows.

On laying out a series of afternoon isothermal lines, it appears that they are closer together in the shallow water than in the deep water, the bottom in the shallower water being in general colder than at the same depth in deep water, though, of course, in the very shallow water, where the surface was several degrees hotter than elsewhere, the whole of this very shallow water was warmed up, and was hotter than water at the same level elsewhere. The rate of change of temperature downwards was very regular, from a depth of from 5 to 6 yards to the bottom. At the depth of 5 to 6 yards it changed more rapidly, and from that up to the surface was the region that was affected by the diurnal changes of temperature. During the day the upper layers in this region became much hotter, and during the night the whole of this region gradually became of the same temperature throughout. The depth of this region was observable, during the days I observed it, by the variation in the rate of change of temperature that occurred at this depth; the change of temperature was more rapid here than in either the subjacent or in the immediately superincumbent layers.

From the rate of decrease of temperature in the superficial layers I calculated that the coefficient of absorption of heat per yard was ·71, but as it is known that this is very different for different rays of the spectrum, it is probable that the coefficient

of absorption of the first layers is very much greater. I had not any sufficiently accurate method of measuring the temperatures at near points to determine the rate of change of temperatures for small distances near the surface, but it was certainly very much more rapid than even at a short distance below the surface.

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EXPERIMENTAL SCIENCE IN SCHOOLS AND UNIVERSITIES

[From *Nature*, vol. 35, 1886-7.]

PROFESSOR G. F. FITZ GERALD, as Vice-President of the Dublin University Experimental Science Association, delivered an address at the opening meeting, held on November 23, in the Museum Buildings of Trinity College, under the presidency of the Rev. the Provost, on "Experimental Science in Schools and Universities."

Professor Fitz Gerald, at the outset of his address, dealt with the history of Universities, and showed how they gave such preponderance to book as against experimental knowledge. That had led, the Professor continued, to a dual system of education—the professional and the commercial. That gap between the classes was much to be lamented, and necessitated, from a political point of view, the desirability of having all classes educated in the same institutions. The commercial classes would not, however, enter the Universities at present, because they required to be taught useful subjects, and they would not learn the Latin and Greek now required in our Universities. From the political side of the question, he thought, they had got these results—that they must be content to have useful subjects taught in their schools and Universities, if the schools and Universities were to be used by the large body in the country who were willing and able to pay for it. What they must have, if possible, was a single school and college system for all classes of the community who were able

to spend the first twenty years or so of their life in education; and they ought to have a system that was complete, which gave a training to those who could not afford to go on the whole length up to twenty years, and which ought to be able to train those who desired to go on for the higher culture. Returning to the education side of the question, he insisted that almost the whole importance was as to how the subject was taught. He thought the use of the Latin Grammar had been reduced to a very good system; but he thought it was perfectly evident from the course that things were taking and the reasonableness of things, that they must teach their youth some knowledge of science. People who felt responsibility in the matter were being more and more convinced that it was not right for them to allow their children to grow up ignorant of the laws of the world in which they live. Others made answer to that, that they left those laws of the world to the doctors. But how were they to know under what circumstances it was well to consult a specialist? It was very necessary for us to have a knowledge when we required to consult a doctor. Hundreds of people were killed by ignorance of the fact that dirt was the cause of disease. That was a very elementary subject. Nevertheless, people were dying every day from ignorance of that very fact; and, unless they were taught to believe in the fact that there were laws of Nature, they would not believe that dirt was the cause of disease, because they saw some people living in dirt and yet not the victims of disease. He thought that time for teaching science must be found for these two reasons—it was necessary that our youth should learn the laws of the world in which they live, and that they also should learn how to discover those laws. Unless our people were taught the laws under which plants and animals were best grown, the people of other countries would rival them in the manufacture of butter and beef, and the result would be that our people must starve. Another advantage of such training was to prevent superstition such as that of the people of Spain, who preferred the use of charms as a safeguard against cholera to the cleansing of their

wells. All the classes of the country required this training—they would die without it, so they must have it.

Having shown that the cultivation of Latin and Greek was originally with the view of acquiring the information contained in the ancient books in those languages, the Professor combated the five reasons formulated by the German professoriate as to why they thought that the cultivation of Latin and Greek was so important, observing, with regard to the fourth reason—that these languages were the best varied exercise in thinking—that if the connexion between words and ideas was a thing that must be taught in every system of education, his impression was that that would be a great deal better attained by describing accurately and thinking out the consequences of physical experiment. In choosing the sciences that they should teach, there were three conditions that should be fulfilled. First of all, the sciences chosen ought to be within the grasp of children, because it was highly important that the science begun with childhood should be continued on in the University days; secondly, it ought not to require any expensive apparatus, because schools and people who trained children could not be expected to buy elaborate apparatus, and children could not be expected to work with them satisfactorily; and, thirdly, he thought the sciences should be chosen so as to be concerned with a large number of the laws of the world in which we live. There were two large branches of science which included nearly all the laws of the world, namely, the physical and the biological; and, therefore, he thought it would be desirable to choose two sciences—one on the physical and one on the biological side, so that children might learn something about the laws of living things, and something about the laws of physical things. He therefore suggested chemistry and botany, and he thought the whole weight of their efforts should be devoted to trying to get the children in schools to learn the elements of chemistry and the elements of botany, for there were no other two sciences the elements of which were almost similar, and at the same time there were no other two sciences that led up to a greater

number of the laws of life, nor that gave a wider or more extended view of the laws of the world in which we live. The objections to the present system of teaching a knowledge of experimental science was that it almost entirely concentrated the person's attention upon phenomena instead of upon reasoning. Therefore, in choosing their system of teaching, all their weight ought to be thrown into making sure that their plan had the effect of making the child learn to think a good deal. Another thing they had to consider was the enormous time that children were made to remain in school without being engaged in anything except mischief. He thought a child should not spend more than four hours a day at literary work. Well, that occupied but a small part of a child's day; and one of the great advantages of having experimental subjects introduced into school teaching would be that they were subjects at which a a child could work without experiencing very much fatigue. He could not help calling attention to the flagrant abuse of the teaching of experimental science in Irish schools. Experimental science in Irish schools was very nearly the same as snakes in Iceland. Having pointed out the fallacy of an examination—as exemplified in the Intermediate Education system—that was satisfied with a reading of the musical signs unwedded to a knowledge of the sounds they represented, the Professor said it would be an enormous advantage if the Intermediate Commissioners could be induced to keep up a peripatetic system of periodical examinations that would insist upon practical knowledge. That, however, should not interfere with the giving of papers also. After observing that it was at the present time impossible to carry out a proper examination in laboratory work, and stating that he considered it would be very desirable that the actual work in the laboratory and analyses in practical subjects should count towards the University prizes, Mr. Fitz Gerald said he considered that the present system of analysis was not very satisfactory, and he urged the introduction of a system that would teach chemistry practically. Though that might be harder to teach than Latin and Greek, it would not be

so if they had a system worked out and teachers to promote it, and it would have the inestimable advantage that, in addition to training the child to think—which he thought it would do equally well with Latin and Greek—it would teach him the laws of things, and how to see and learn the laws of things. It would also teach the child to use language to express real ideas, and not merely phrases. They would also learn a good deal more of the laws of language from a modern language that they learned with the grain than they would by learning an ancient language against the grain. He thought that literature and history were co-ordinate with science, and they certainly ought to be a large part of education. Literature and history were grievously neglected in the present day—practically they had no place, and that was substantially because Latin and Greek were supposed to be a literary education. One of the reasons was that those subjects were hard to examine in, but there was an easy way out of that difficulty in Universities. They need not examine, but they could require attendance at lectures—attendance on good lecturers; and the student would pick up more culture and would be obtaining a better literary education from hearing a good lecturer and being inspired by his enthusiasm than he would get by learning off one of Shakespeare's plays, and answering it at an examination. Those two aspects of education, the literary and scientific, were often put in opposition, just as the freedom of the individual and the power of the State to control the individual were very often set up in opposition to one another; but he did not think any one would believe that opposition really arose, for the freest States were those in which the power of the State was the strongest. In conclusion, he would say that we must equip our youth for the battle of life physically and ethically. The present is a great crisis in Irish education. There is danger of science schools starting, and all the evils of dual education. There are a large body who like Latin and Greek, because they exclude literature and history. These are to be fought tooth and nail. There are those who would sacrifice the rising generation on an altar of

so-called culture to starve and die, with their only comfort that they can describe their agony in well-expressed phrases. There are those who would grind all soul out of mankind in a mill of manual labour, constructed on scientific principles. All those are to be guarded against. We must have literature and history. We must have knowledge of the laws of the world in which we have to work. We can have both if we will but work out a reasonable system of education, instead of pretending that the lop-sided corpse that occupies our schools and Universities is a well-developed, symmetrical giant.

42

ON THE ACCURACY OF OHM'S LAW IN ELECTROLYTES

[Conjointly with Dr. F. T. Trouton, F.R.S. From the *Report of the British Association*, 1886.]

SOME preliminary experiments were begun in the spring of this year, and have been carried on from time to time since, with a view to determining how far Ohm's law may be relied on in the case of electrolytes.

Though as yet no very high limits of accuracy in the experiments have been attained, owing to causes which will be later on explained, still it is hoped that from the experience already gained most of the difficulties have been got over, and that soon results of a much greater degree of accuracy will be arrived at.

The method adopted is that described in the British Association Report for Glasgow, 1876, which Professor Chrystal, in connexion with Clerk Maxwell, employed in the case of metal conductors, namely, that of a Wheatstone's bridge, two of the arms of which are of about equal section and resistance, while the remaining two, though of equal resistance, are of very different sections. If Ohm's law be not true, the point of balance of the bridge varies with the amount of current passing through it. Balance having been obtained, the battery-power is altered, and if balance still subsists, the deviation from Ohm's law, if there be any, is such as cannot be detected with apparatus of the sensitiveness employed.

The great difficulty in experimenting in this way is the the change in the current alters the temperature of the two arms of unequal section very differently, and proportionately also their resistances. While the thick conductor alters little

in temperature, the thin one alters considerably. Thus the point of balance is changed, even though Ohm's law be true. To avoid this effect it is necessary, having balanced with the large current, to immediately pass the smaller current before the temperature can sensibly alter. As this requirement is practically impossible to satisfactorily fulfil, the method employed is a rapid alternation of a large and small current. If Ohm's law be not true, a balance in this case can only be apparent, and on reversing the direction of one of them, the two currents which before neutralized each others' action now conspire to deflect the galvanometer. The mode of experimenting is to find balance with a large and a small battery alternately in circuit, and rapidly enough for the temperature during the smaller current to be sensibly the same as while the larger one is acting, then to change the direction of one of the currents and to again balance. The distance is observed between the two points of balancing, but if the bridge still balances it is assumed for the purposes of calculation that the distance is what the galvanometer employed only just detects. From this, from the two currents, and the section and resistance of the thin conductor, h is calculated, where $e = rc(1 - hc^2)$, as is explained in the above-mentioned Report.*

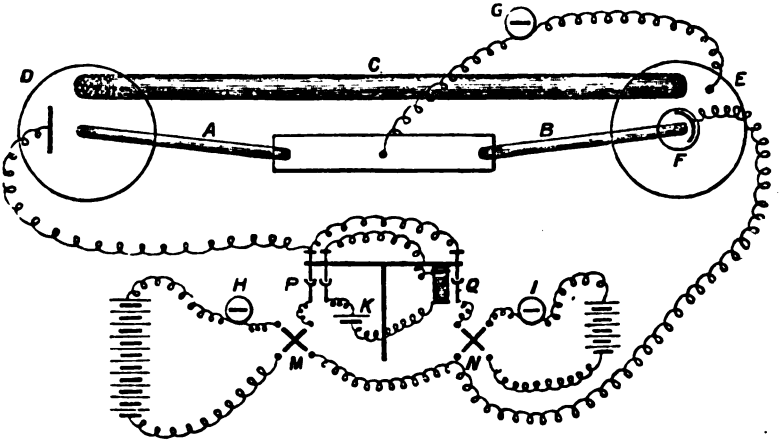
The liquid, chosen for experimenting with, was a solution of copper sulphate in water. The equal arms consisted of two glass tubes bent at right angles at each end, A and B, arranged siphon-like, and contained the solution. They dipped into a long narrow trough, as shown in the diagram. The arm of large section C, a tube 119 cm. long and 2·38 cm. in internal diameter, reached from the bowl D into which the tube A also dipped, to the bowl E. The small arm was a hole ·055 cm. in diameter, drilled in the side of the beaker F, ·063 cm. in thickness, into which the tube B dipped.

The battery poles consisted of copper plates, one in the beaker, the other in the bowl D. The contact-breaker was a T piece worked by an electromagnet and cell K, as shown in

[* e is electromotive force, c current, and r resistance for very small currents.]

the diagram. One of the batteries contained ten Grove cells, the other five, and both could be thrown in circuit in either direction by means of the reversing keys M and N. By proper adjustment of the mercury cups P and Q one of the batteries went in circuit, just as the dipper on the other side, coming out of the mercury, threw the second battery out of circuit. By means of the galvanometers H and I it could be seen, during the course of an experiment, whether the contacts were working properly or not.

One of the copper wires leading to the galvanometer dipped into the bowl E, the other into the trough, and could be moved



up and down it in order to obtain balance, the distance moved being read off a scale along the trough. There was always more or less of a permanent current through the galvanometer, even though the poles were both of copper; this, especially as it was liable to sudden changes in amount caused by moving the trough pole or by accidental shaking, necessitated the employment of the galvanometer in a comparatively insensitive condition. Various devices were tried with a view to avoiding this current, such as electroplating the poles, using chemically pure copper sulphate, or employing for poles either end of a broken wire, so as to have, if possible, metallically like poles;

these were found to give an indication of current even in distilled water.

The resistance of the smaller arm was about 800 ohms, but owing to its smallness varied greatly with the current through the change of temperature. A good deal of trouble was experienced through the resistance of the smaller arm, at times, being less when the current passed in one direction than when it passed in the other. This occurred so often that until its cause was understood, and steps thus could be taken to prevent it, there was little hope of completing the experiments. After various attempts it was discovered to be due to the density of the solution on the outside of the beaker in the bowl being slightly different from that inside, so that the density in the hole, and consequently its resistance, depended on the direction of the current, the electric transfusion of the liquid always changing direction with the current.

A number of experiments were made of a more or less satisfactory nature; in all no difference in the position of balance was found on reversing one of the batteries. Taking the difference then to be what the galvanometer would just detect, h is calculated to be less than 10^{-4}. This is, of course, very large. The limit Professor Chrystal reached in his experiments with metal conductors was less than 10^{-19}.

With a view to reaching a higher limit a smaller hole was next tried. It was ·017 cm. in diameter, drilled in a plate of glass ·022 cm. thick. Its resistance was about 2700 ohms, so that a longer tube had to be used for the large arm. The balance was always found to be very different when one of the batteries was changed in direction. However, this was probably entirely due to the difference in temperature during the small and large currents, for h calculated from this was not even as small as in the first experiments; also the difference between the balance points varied with the speed of the contact-breaker. Unfortunately the contact-breaker, which was adjustable in its rate of vibration, had reached its limit of speed, so that a completely new arrangement has now to be employed.

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ON OHM'S LAW IN ELECTROLYTES

[Conjointly with Dr. F. T. Trouton, F.R.S. From the *Report of the British Association*, 1887.]

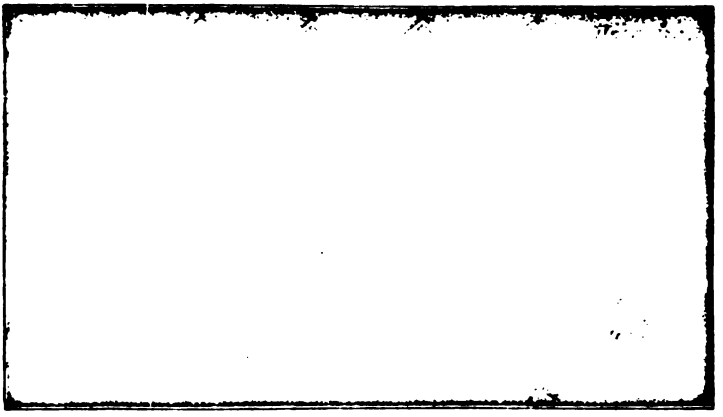
THE result of our experiments up to this is to ascertain that "h" in $[r = r_0 (1 - hc^2)]$ is certainly less than 5×10^{-6}.

This time last year it was hoped shortly to attain much greater accuracy than had then been reached, but it is clearer now than then what a difficulty the "heating effect" is.

This inherent difficulty in studying Ohm's Law for liquid electrolytes, as compared with metallic conductors, may best be understood by considering the increased difficulty which would be introduced in the determination for metals if the wire were to be immersed in an electrically non-conducting liquid. When the wire loses heat, chiefly by radiation, as when in air, its general temperature is so much above the temperature of surroundings that its rate of cooling may be considered constant for the small changes of temperature which can occur, if the alternations from the larger to the smaller current be sufficiently frequent. The temperature thus rises at a constant rate while the larger current runs, and so falls again during the time the smaller current is on, the average temperature in both cases being the same. The upper part in the diagram illlustrates this.

However with the same frequency in changing currents, the average temperature, while the larger current runs, may be made very different from the average temperature while the smaller current is on, by immersing the wire in liquid; for the

general temperature of the wire can then be only slightly above the temperature of its surroundings, so that the rate of cooling can no longer be considered the same throughout. The lower part of the diagram is intended to roughly represent this. The temperature curve is concave to the lower side while the larger current runs, but is convex as the wire falls in temperature during the period of the smaller current. The "temperature effect" if thus introduced would have to be met by increasing the speed of the contact-breaker; or otherwise smaller currents should be used, which of course would diminish the refinement in the determination of "h" correspondingly.



Now this is similar to the case of a liquid electrolyte, the smaller arm of the bridge rapidly losing heat by convection, from its necessary proximity to the larger bodies of liquid at both ends. The actual rise in temperature in one experiment was ascertained, through the increased resistance, to be much less than ten degrees; while in some of the determinations made with metals the wire fused during the experiment, so high a temperature was reached.

The greatest speed found necessary for the contact-breaker in the determination with metal conductors, when "h" was ascertained to be less than 10^{-12}, was about 100 per second.

The fastest of the three forks we have successively employed is about 160 per second, but a much faster fork has been prepared, though as yet it has not been got to work satisfactorily.

The diameter of the smaller arm has been reduced considerably since last year. One hole of ·0027 cm. was bored with a specially prepared needle in extremely thin mica. So that not much more can be done in this direction. The density of the greatest current which could be used through this without "heating effect" was about ten ampères per square centimetre.

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ON THE ACCURACY OF OHM'S LAW IN ELECTROLYSIS

[Conjointly with Dr. F. T. Trouton, F.R.S. From the *Report of the British Association*, 1888.]

EXPERIMENTS have been made with the faster fork alluded to in last year's report, with the result of bringing the determination to about twice the degree of refinement then mentioned as attained, or "h" to be less than 3×10^{-6}. The rate of this fork (about 290 vibrations per second) is nearly twice that of the previously employed fork (160 per second). From the experiment described below the possible increase in refinement obtainable by doubling the rate appears to be about fourfold, or twice that obtained.

Considering that the difficulties of working increase greatly with the rate of the fork, it was thought more advisable for the further prosecution of the investigation to try some other method of breaking the contacts than to prepare a still faster fork.

A commutation arrangement attached to the spindle of a small magneto has been employed with fairly satisfactory results. As great a refinement, however, has not yet been reached with it as was attained with the fork method,* chiefly through the difficulties connected with running the magneto uniformly; but we hope soon to have storage cells in the laboratory, and thus to get over this difficulty.

* A very unsatisfactory experiment with the magneto at a speed of about 400 vibrations per second gave, as the value of h, $1{\cdot}25 \times 10^{-6}$.

The magneto arrangement, however, on account of the facilities for changing the speed, was found admirably adapted for confirming the supposition that the observed deviation from Ohm's Law is due to "heating effects," for the deviation always tended to disappear as the speed of contact-breaking was gradually increased; also for investigating the manner in which the minimum value of "h" obtainable varies with the speed of contact-breaking. This was found to be, approximately inversely as the square of the speed.

45

ON THE THERMODYNAMIC PROPERTIES OF SUBSTANCES WHOSE INTRINSIC EQUATION IS A LINEAR FUNCTION OF THE PRESSURE AND TEMPERATURE

[From the *Proceedings of the Royal Society*. Received January 11, 1887.]

PROFESSOR RAMSAY has communicated to me that he and Mr. Young have found that within wide limits several substances in the liquid and gaseous states have the following relation connecting their pressure (p), temperature (T), and specific volume (v),

$$p = a\mathrm{T} + b,$$

where a and b are functions of v only.

Now in this case the following are the forms that the thermodynamic equations assume. T is temperature, and ϕ is entropy, and c and e are functions to be investigated, c being $= d\mathrm{I}/d\mathrm{T}$, where I is the internal energy, and $e = d\mathrm{I}/dv$.

Then

$$\mathrm{T}d\phi = cd\mathrm{T} + (e + p)\,dv.$$

From this, as $de/d\mathrm{T} = dc/dv$, we have $dp/d\mathrm{T} = (e + p)/\mathrm{T}$.

But $dp/d\mathrm{T} = a$ by the intrinsic equation, and is a function of v only; $\therefore\ c = -b$, which is a function of v only; $\therefore\ de/d\mathrm{T} = 0$; $\therefore\ dc/dv = 0$; $\therefore$ is c a function of T only, and $d\mathrm{I} = cd\mathrm{T} + edv$ gives

$$\mathrm{I} = \int cd\mathrm{T} + \int edv = \gamma + \lambda,$$

where γ is a function of temperature only, and λ a function of volume only.

Similarly, $$T d\phi = c dT + a T dv;$$

$$\therefore \quad d\phi = \frac{c}{T} dT + a dv;$$

$$\therefore \quad \phi = \int \frac{c dT}{T} + \int a dv,$$

$$\phi = \Gamma + \alpha,$$

where Γ is a function of temperature and α of volume only.

Hence we see that c, the specific heat at a constant volume, is a function of the temperature only, and the internal energy and the entropy can be expressed as the sums of two functions, one a function of the temperature only, and the other of the volume only.

For the specific heat at constant pressure we have

$$C = c + (e + p)\frac{dv}{dT}$$

$$= c + aT \cdot \frac{-a}{Ta' + b'},$$

where $a' = da/dv$ and $b' = db/dv$;

$$\therefore \quad C - c = -\frac{Ta^2}{Ta' + b'}.$$

In the case of the particular values of a and b that Professor Ramsay has suggested to me, when the intrinsic equation assumes the form

$$p = \frac{RT}{v - v_0} - \frac{\mu}{v^n},$$

and when consequently

$$a = \frac{R}{v - v_0}, \qquad b = -\frac{\mu}{v^n},$$

we have $$= \mu v^{-n},$$

and $$I = \gamma - \frac{(n-1)\mu}{v^{n-1}},$$

$$\phi = \Gamma + R \log (v - v_0),$$

$$C - c = \frac{TR^2}{TR - n\mu (v - v_0)^2 v^{-n-1}}.$$

It would be most important if by some method, Kœnigs' for instance, or by inserting a small microphone into a tube, the velocity of sound in substances in various states could be accurately determined, as that would enable us to determine C and c respectively.

46

CLAUSIUS' FORMULA FOR THE CHANGE OF STATE FROM LIQUID TO GAS APPLIED TO MESSRS. RAMSAY AND YOUNG'S OBSERVATIONS ON ALCOHOL

[From the *Proceedings of the Royal Society*. Received March 14, 1887.]

CLAUSIUS, in Wiedemann's "Annalen," vol. 14, 1881, pp. 279–290, and "Phil. Mag.," vol. 12, 1881, p. 381, and vol. 13, 1882, p. 132, has given an empirical formula for calculating the relation between the volume, pressure, and temperature of a substance in both liquid and gaseous states. The equation he gives is a continuous one for an isothermal, and he determines the pressure at which evaporation takes place by considering that the work done in the transformation from liquid to gas at a constant pressure must be equal to what would be done if the transformation took place along the continuous isothermal. He requires, for convenience in applying this to actual cases, to calculate the values of certain rather complicated exponential functions, and has published tables of their values which greatly facilitate the work of comparing his formula with observations of vapour-pressures at different temperatures. He has compared his formula with determinations of vapour pressure, etc., by Andrews and Regnault of carbon dioxide, and by Regnault and Sajotschewsky of ether, and with Regnault's experiments on water, and has shown that they agree very well. He has also, by help of his formula, calculated the critical temperature for water, and finds it to be about 332° C., and the critical pressure to be 134 atmospheres.

Professor Ramsay has kindly furnished me with his and Mr. Young's observations on alcohol, and I have compared them with Clausius' formula, with which they agree very well.

The formula Clausius has given may be described as follows:—

The relation connecting the volume, pressure, and temperature of a substance can be expressed by the formula

$$\frac{p}{RT} = \frac{1}{v-a} - \frac{1}{\Theta(v+\beta)^2}.$$

In this R, a, and β are constants for each substance, p is the pressure, v the specific volume, T the absolute temperature, and Θ is a function of the temperature which vanishes with T, and for which Clausius has given the formula*

$$\frac{\Theta_c}{\Theta} = (1+b)\left(\frac{T_c}{T}\right)^n - b,$$

in which b and n are constants for any one substance, and T_c and Θ_c the values of T and Θ at the critical temperature.

From a consideration of the isothermals represented by Clausius' formula it is easy to show that the critical isothermal, for which two of the tangents parallel to the axis from which pressures are measured coincide, gives

$$\Theta_c = \frac{8}{27(a+\beta)}.$$

As the combination $a+\beta$ occurs frequently, Clausius denotes it by γ. He expresses the specific volumes of the saturated liquid and gas by σ and s, and uses w and W for $\sigma - a$ and $s - a$ respectively. He also uses the symbol $\Pi = P/RT$ where P is the saturated vapour-pressure, and subscribes c (thus Π_c) to

* There is a misprint of $\frac{\Theta}{\Theta_c}$ for $\frac{\Theta_c}{\Theta}$ on the last line of the text, formula 7, of p. 135 of Clausius' second Paper in the "Phil. Mag.," vol. 13.

express the value of any of these quantities at the critical point. Hence we get

$$\Pi_c = \frac{1}{8\gamma}, \qquad W_c = w_c = 2\gamma.$$

I have assumed as the result of observation that $T_c = 516{\cdot}5$ and $P_c = 49{,}000$ mm. This latter value is probably a little too large; but, as there seems some uncertainty as to its value from the experiments, from which it is very difficult to approximate accurately to the actual position of the critical point, I have thought this value sufficiently accurate. I have calculated the result of making changes in this value, and any variation within limits allowable by the experiments does not materially affect my results.

The values of these constants for alcohol as determined from the observations are

$$\begin{aligned} R &= 1351{\cdot}35, \\ T_c &= 516{\cdot}5, \\ P_c &= 49{,}000, \\ \alpha + \beta = \gamma &= 1{\cdot}780, \\ W_c = w_c = 2\gamma &= 3{\cdot}560, \\ \Pi_c = \frac{1}{8\gamma} &= 0{\cdot}07023, \\ \Theta_c = \frac{8}{27\gamma} &= 0{\cdot}1664, \\ b &= 0{\cdot}9118, \\ n &= 1{\cdot}3462. \end{aligned}$$

I have found that constant values for α and β do not satisfy the observations accurately, and that α varies from 1·087 at 0° C. to 0·184 at 240° C., as I explain further on. I calculated b and n so as to make the saturated vapour-tensions correct at 0° C. and 100° C. The process of calculation I adopted was to

calculate Π/Π_c, at 10° C., and 100° C., and then from Clausius' tables obtain the corresponding values of Θ/Θ_c. This gave two equations, to determine b and n by means of

$$\Theta_c/\Theta = (1 + b)(T_c/T)^n - b,$$

the equation for n being

$$\frac{\frac{\Theta_c}{\Theta} - 1}{\frac{\Theta_c}{\Theta'} - 1} = \frac{\left(\frac{T_c}{T}\right)^n - 1}{\left(\frac{T_c}{T'}\right)^n - 1}.$$

This was solved by trial and error and then b calculated. Having determined b and n, the value of Θ/Θ_c for any temperature may be easily calculated, and thence the corresponding values of Π/Π_c, W/W_c, and w/w_c obtained by interpolation from Clausius' tables. From these P, W, and w are calculated and compared with observation. It is from this comparison of w with the volume of the liquid at various temperatures and pressures that it is evident that a is not constant, for the difference between w calculated and σ as observed is by no means so.

The following table exhibits some of my results:—

	0° C.	100° C.	200° C.	240° C.	242·5° C.	243·5° C.
P observed . . .	12·24	1695	22434	46339	—	—
P calculated . .	12·24	1695	22106	46172	—	49000
W	30·210	284·5	19·09	5·43	4·26	3·56
s	29·046	284·4	19·85	5·83	4·62	—
w	0·177	0·389	1·003	2·43	2·99	3·56
σ	1·264	1·390	1·796	2·614	2·925	—
$\sigma - w = a$ . . .	1·037	1·001	0·793	0·184	− 0·065	—

In some of these cases, as for example for W at 0° C., it is very difficult to interpolate accurately into Clausius' tables; and similarly for the values of w near the critical point; and I

consequently do not attribute much accuracy to these values. On the whole, however, I think that, considering the enormous range of values to be represented by the formula, it is most remarkably accurate. When we compare the calculated and observed volumes of the liquid, in which case a is of importance, we find that no constant value for a can make them agree, for a obviously diminishes with increased temperature, and near the critical point the value of a for the liquid and gaseous states is not the same. All this means of course that Clausius' formula does not apply accurately to the case of alcohol.

Clausius has not, as far as I can find, applied his formula to calculate the volumes of liquids, and without doing so the want of constancy in a would not appreciably affect the result. Messrs. Ramsay and Young have made observations of the volume of the liquid at various temperatures and pressures, and I have compared some of their results with the formula. In this way it can be seen that a must be made a function of the pressure as well as of the temperature. I have calculated the values of $v - a$ at certain temperatures and pressures, and find at 110° C.

Pressure	2362	60,000
$v - a$ (calculated)	0·409	0·383
v (observed)	1·417	1·3925
a	1·008	1·0095

At 200° C.

Pressure	22,434	60,000
$v - a$ (calculated)	1·003	0·858
v (observed)	1·793	1·720
a	0·793	0·862

At 240° C.

Pressure	47,500	60,000
$v - a$ (calculated)	2 166	1·641
v (observed)	2·468	2·169
a	0·298	0·528

From this it is evident that a diminishes with increased temperature, and increases with increased pressure.

Notwithstanding this, that Clausius' formula does not at all accurately represent the state of the liquid, there is no doubt that it gives a wonderfully accurate general representation of the more important features of the change of state. In this respect it is of enormously higher value than the formulæ that only give the relations connecting the temperature and pressure of saturated vapours. In addition to this, which it certainly gives in a rather complicated way, it gives the state of the liquid and gas before and after as well as during evaporation, and enables us to calculate points on the theoretical continuous isothermal connecting the liquid and gaseous states. I have calculated enough of these points to roughly sketch in these curves that cannot be made the subjects of experimental investigation by the usual methods, owing to the instability of the states they represent.

Clausius' equation may be put into the following form by assuming $p/\mathrm{RT} = y$, and $v - a = x$:—

$$xy(x+\gamma)^2 = (x+\gamma)^2 - \frac{x}{\theta}.$$

From this it is evident that an isothermal is a quartic curve having asymptotes $y = 0$, $x = 0$, and $x + \gamma$ a double asymptote at a cusp at infinity, so that the point at infinity on this line is a multiple point of a high order.

If we calculate the positions of the points of tangency of

tangents parallel to $y = 0$, for which consequently $dy/dx = 0$, we have the cubic equation

$$(x + \gamma)^3 = \frac{2x^3}{\Theta};$$

when two of its roots are equal $\Theta_c = \frac{8}{27\gamma}$, and this determines the critical isothermal. The quartic consists of three branches. One, a serpentine branch, lies in the positive region of x and mostly of y, and is the only branch of physical interest at present. The other two branches lie entirely in the negative region of x and y. One of these is somewhat parabolic, and lies between $x = 0$ and $x = -\gamma$ asymptotic to both of them, the other is hyperbolic and asymptotic to $x = -\gamma$ and to $y = 0$. The always real solution of the cubic, that determines the points where $dy/dx = 0$ for positive values of Θ and γ, is a point on the parabolic branch of the curve that lies between $x = 0$ and $x = -\gamma$. The other two roots, when real, determine the highest and lowest points on the serpentine part of the curve that lies in the positive region of x.

FIG. 1.

The above diagram represents the general features of this class of curve. It is the particular case of the quartic

$$(\mathrm{L} + a\mathrm{M})^2(\mathrm{M}^2 - \mathrm{LN}) = b\mathrm{LM}^3,$$

where L, M, and N are lines, of which M is here the line infinity

and L and N are at right angles. In the general case this quartic is a continuous curve with a cusp at the intersection of L and M, and L + aM as the cuspidal tangent, while L is a tangent to another branch that passes through the same point.

In our case its general features are something like this.

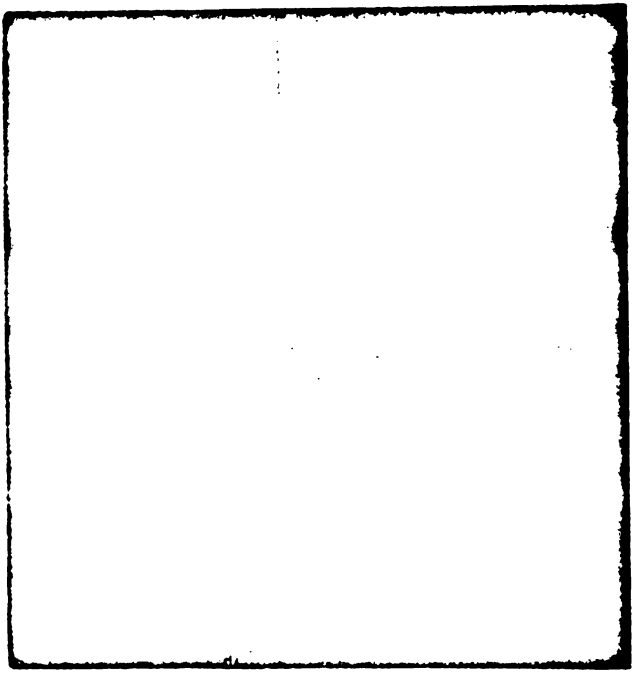

Fig. 2.—General Feature Diagram.

There is generally a double inflexion in the part of the curve outside the triangle L, M, N and the particular one of a series of such curves for which the two inflexional tangents coincide is what corresponds to the critical isothermal in a gas. It is only what corresponds to the part outside the triangle that is of physical interest.

In the particular case of the curves representing the isothermals of alcohol, the negative parts of the curve lie at a

very great distance from the origin compared with the dimensions of the serpentine part of the positive branch, so that it is not easy to represent them both in the same figure. I have calculated several points on the isothermals corresponding to 110° C., 207·5° C., and 243·5° C. 207·5° C. is the isothermal

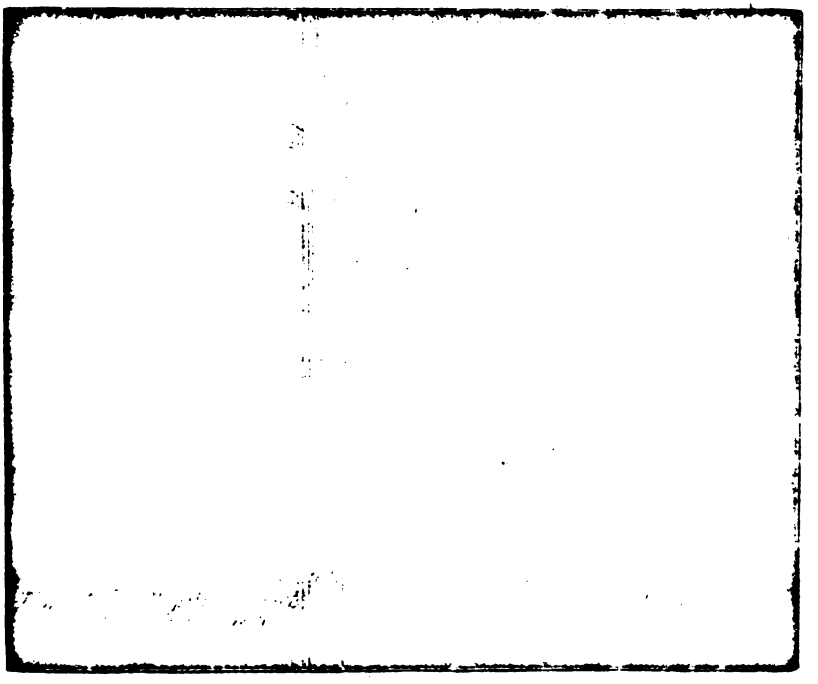

Fig. 3.

that just touches $y = 0$, while 243·5° C. is the critical isothermal. The dotted lines represent the saturated vapour-tensions for which the areas included in the loops of the curve above and below are equal. The points numbered 1, 2, 3, 4 are specially noticeable points on the curves, so numbered in the general feature diagrams.

The isothermal 110° C. goes down at the point 1 entirely outside the diagram to a pressure of − 246,800 mm., and

intersects its vapour pressure line outside the diagram to the right at a volume of 206. The negative parabolic branch of this isothermal only comes up at the point 3 to a pressure of – 4,442,000 mm., while the corresponding points of 207·5° C. and 243·5° C. come only to – 4,044,000 mm. and – 2,743,000 mm., so that they are very far off. In the isothermal 207·5° C. the point 4 lies at about $x = -32$, $p = -32{,}000$, so that it is not very far off.

What strikes me as most remarkable about these curves, and other than what one might have expected, is the very great distance to which the point 1 descends at ordinary temperatures. It would be interesting to compare the forms of these parts of the curve for several liquids, and see whether there was any connexion between it and the capillarity.

47

ON THE MEASUREMENT OF SMALL PRESSURES

[Conjointly with Professor J. Joly, Sc.D., F.R.S. From the *Scientific Proceedings of the Royal Dublin Society*. Read April 18, 1888.]

THE difficulty in measuring small pressures is not on account of the smallness of the forces concerned: a pressure of the millionth of an atmosphere is about the weight of a milligram per sq. cm., and this on any considerable area is quite a large force compared with the weights measured every day in balances. The difficulty arises from it being so hard to make the surface pressed on freely movable. We may use fluids in tubes—in horizontal or nearly horizontal tubes, for instance—but the sticking of the surface of the liquid and the tube, due to capillarity, which becomes serious on account of the necessity for using small tubes, if the liquid is to stand with free surfaces nearly at right angles to the tube, when it is nearly horizontal, has seriously interfered with this method of observation. Herr Töppler has recently used a liquid in the bend of a tube bent at a very obtuse angle, and has observed the motion of the surface by means of a microscope. This method seems capable of considerable accuracy. The capillary sticking might be entirely obviated by making the tube large. To whatever extent it exists it would generally diminish the effect of pressure, and its irregularity of action would introduce an unnecessary uncertainty into the measurement. In the case of water, the difference of level due to a pressure of the millionth of an atmosphere is the hundredth of a mm., an amount that can be easily measured by means of a microscope. A float on the

surface with a fine mark attached would be a convenient object to observe with the microscope. The difficulty in this case is to keep the float steady, but it could probably be anchored at the junction of two crossed spider lines, or by suspending part of its weight by a stretched spider line. With a pretty large float—and there is no reason in many cases why it should not be large—there would be plenty of force to turn a light mirror; and so a spot of light, instead of a microscope, might be used to observe changes in the level of the liquid. A method depending on changes in the position of Newton's rings might also be used. In all methods when liquids are used, the vapour pressure of the liquid is one of the great difficulties in measuring the pressure of the rest of the gas, and can only be reduced by using liquids that have a very small vapour pressure. In some cases we can practically get rid of it by only opening temporary communication between our measuring apparatus and the gas, whose pressure we wish to determine, and not allowing time for any serious amount of diffusion to take place. At very low pressures, as the flow of gas through even relatively large tubes is of the nature of diffusion, a great additional difficulty is introduced.

Another method of measuring very small pressures is to observe the position of the centre of a flat elastic membrane when subject to a difference of pressure at its opposite surfaces. A flat film of indiarubber, such as is used in toy balloons, stretched across a circle 5 cm. in diameter, can easily show, when observed by a microscope, a difference of pressure of the millionth of an atmosphere. The very smallest difference of pressure will move a flat film, and thin flat films of glass can be made which can be moved by very small pressures. There is an objection to using inextensible films like glass because of the uncertainty as to what point moves most, but there is a great advantage in their non-liability to diffusion or evaporation, both of which faults exist in indiarubber films. We have not made sufficient experiments on glass films to speak with any degree of confidence as to their being capable of measuring such small pressures as the millionth of an atmosphere. If a lens were

placed near them, and displacements of Newton's rings observed, it seems most likely that a very much less pressure could be detected. Of course soap-bubble films might in some cases be employed, though they are liable to all the faults of indiarubber films, and to others of their own, such as the difficulty of attaching a suitable pointer to be viewed through the microscope, their want of permanence, especially if vertical, and the danger of dust, liquid, etc., deflecting them if they are horizontal. An aneroid barometer is practically a film such as we are suggesting, and one on a large scale observed with a powerful microscope would probably be a very efficient measurer of small changes of pressure. A film of this kind, which had sufficiently little rigidity to be deflected by a very small change of pressure, would not, in general, be strong enough to support high pressures, and if a column of liquid be added on top of it to assist it, the surface may not be in stable equilibrium. For instance, if it is attempted to get over the difficulty of the evolution of hydrocarbons by indiarubber, and the facility it offers for diffusion, by pouring a liquid on top of the film and increasing the air pressure below, so as to keep the film flat, it will be found that the film is no longer stable, but buckles up in some places and down in others. If the pressure below be not sufficient, the liquid gradually extends the indiarubber until it bursts it. By choosing the right pressure for the air below, it would be possible to have the film just near the point of neutral equilibrium, and then any very small change in the pressure would make a considerable one in the position of the film. Stretched films of indiarubber are, however, a nuisance to work with, they tire so rapidly.

A third method of measuring small pressures accurately is by a plan which is practically the same as the balanced barometer tube. If a bell glass be turned over into a liquid, any small change of pressure in the air inside produces a corresponding force on the glass, and if it be attached to a balance this change in pressure may be readily weighed. As this pressure varies as the area of the surface of liquid enclosed,

and the capillarity only varies as its perimeter, by making the vessel large the relative effect of capillarity may be made very small. The only difficulty in this method arises from the very small range of motion of the suspended vessel. In the case of water being the liquid, the motion of the suspended vessel would only be the hundredth of a millimetre for the millionth of an atmosphere; but this should be quite easily detected by means of a microscope, or a moveable mirror, or Newton's rings. This method is liable to the objection that a liquid with a possible, though perhaps small, vapour pressure is present. Another objection to liquids which have necessarily horizontal surfaces is the danger of their absorbing gases superficially, and so changing in density. The only one of these methods that is quite free from all these objections is the one depending on the use of large, flat, thin glass or metal films.

There are several uses to which these methods of measuring small pressures may be put. One of the most important is the determination of the densities of gases by balancing columns of them against one another. This method of measuring the density is quite independent of condensation of the gas upon surfaces, which introduces such difficulties into balloon and displacement methods. Mr. Joly has suggested it as a method of measuring high temperatures, by measuring the density of a known column of air heated to the high temperature. For these purposes, the indiarubber film, we believe, can be got to work satisfactorily. It need only be used as an indicator, to show when the pressures on its two faces are equal. The change in pressure can be measured by a known small compression of the air at one of its faces. Mr. Joly has made some experiments on the possibility of using a horizontal column of dusty air as an indicator to show whether two columns of gas balance one another, but his results were not very encouraging. This method, or one depending on the blowing about of fine fibres, such as silk, cobwebs, etc., is, however, we believe, capable of being used to determine when the pressures in two vessels are exactly equal.

One of the most desired measuring instruments at present is one by which it may be possible to measure very small pressures accurately when the total pressure is very small. The M'Leod gauge is almost the only apparatus at present in use for this purpose. It has obvious objections. It is doubtful what exactly its indications mean. To what extent is there condensation of gas on the surfaces? To what extent does the mercury vapour condense? The presence of other condensible gases is fatal to its indications.

Some apparatus such as we have sketched out might be used instead of the M'Leod gauge; but, of course, none of them can ever tell us when we have attained to a perfect vacuum. We must be content to compare pressures with that in some standard vacuum: it is evidently impossible by any methods such as we have been proposing ever to measure absolute pressure; we can only by these means measure differences of pressure, and so we can only compare one vacuum with some standard vacuum, such as a potash vacuum, which we provisionally take as a standard.

Note.—During the discussion on this Paper, Mr. Vereker suggested mica as a suitable substance of which to construct films.

48

ENGINEERING SCHOOLS

[From *Nature*, August 2, 1888.]

AT a time when so much is being said about the need for technical education, especially in engineering, the following letter will be read with interest:—

ENGINEERING SCHOOL,
TRINITY COLLEGE, DUBLIN,
June, 1888.

DEAR LORD ASHBOURNE—As you have requested me to draw up a statement of the claims of engineering schools to be recognized by the Civil Service Commissioners as affording part at least of the technical training required of candidates for engineering Civil Service appointments, I send you the following account.

Allow me, in the first place, to state that I am not advocating the claims of our Engineering School here as in any way distinct from that of many other excellent engineering schools that exist. For instance, the Indian Government is so fully convinced of the absolute necessity for a proper technical school training for engineers that it requires all candidates for Indian engineering appointments to go through Cooper's Hill Engineering School; and yet the Home Civil Service do not in any way even recognize the very same technical training given to other students who stay at home as of any value at all.

The instruction given in engineering schools is of two kinds:—

I. Lectures and demonstrations in mathematics, mechanics, physics, chemistry, geology, etc.; and in the theory and practice of engineering, surveying, etc., etc.

II. Practical training—

(*a*) Practical work in laboratories and workshops, in mechanics, machines, physics, chemistry, and field-work in geology.

(*b*) Drawing and office work, including designing, making out specifications, taking out quantities, etc., etc.

(*c*) Practical surveying, and all manner of field work.

(*d*) Inspection of works in progress.

It will be observed what a large and important part of the training given in a school cannot be obtained in an office at all. All the instruction in mathematics, mechanics, physics, chemistry, geology, etc., and in the theory of engineering, and all the important practical laboratory training in these subjects, can only be obtained in a school; and unless an engineer has been properly and *practically* taught these things before entering on his profession, it is almost certain that he will never learn them. In the other, more especially engineering, parts of the course, there are several great advantages in the school course over the office course. In the school, in the first place, the student is under the constant instruction of teachers whose time is devoted to instructing the student, and explaining to him the principles upon which his work depends; and, in the second place, the course of instruction covers as wide a range of subjects as is consistent with teaching each properly. In the office, in the first place, the apprentice has to pick up what instruction he can, and is generally content with a rule-of-thumb knowledge, that may desert him at any really critical juncture; and, in the second place, in any one office the work is yearly becoming more specialized, so that an apprentice will have experience of only a small range of subjects, and, not

being acquainted with the theory of even these, will be incompetent to engage in other work.

There are, of course, certain things such as facility in numerical calculation, and perhaps in the use of field-instruments, acquaintance with the details of specifications with a particular class of work, familiarity with prices at a particular time, and an opportunity of seeing designs carried into execution, which cannot be as well obtained in school as on works. The object of a school being to teach, and of works being to pay, neither can completely supply the place of the other. As a course of technical training for a young engineer, the school course is out of all proportion the more important. What can be learnt from the office course will certainly be acquired, while what can be learnt from the school course will hardly ever be acquired unless learnt before beginning the practice of his profession. In this age of technical education it is practically certain that in a few years no engineer will be recognized as such unless he has had a proper technical school education; just as in the medical profession it has long ago been recognized that, without a proper medical school education, it is impossible for a doctor to learn the many sciences upon which the successful practice of his profession necessarily depends.

Eminent engineers who have had experience of students taught in engineering schools hold opinions similar to those here enunciated. Our late Professor of Engineering, Mr. Crawford, whose engineering experience is world-wide, is of this opinion. Mr. Bindon B. Stoney, Engineer to the Dublin Port and Docks Board, is of the same opinion. Both these have had experience of school-trained students, and think that the proper course for a young engineer to pursue is to go through a course of instruction in a properly-equipped school, and then to go for a year on works. They consider that a year on works is required to complete the education of an engineer, and they think that a short time on works is quite sufficient for a student who has already gone through an engineering school. Mr. Stoney, for instance, takes students who have been through an engineering

school as apprentices for one year, although he will not take untrained apprentices for so short a term.

Foreign Governments in general require all who profess to practise as engineers to go through a proper technical school training; and it is a serious difficulty in the way of English engineers who endeavour to obtain employment on the Continent that, even though they may have been trained in an excellent school, yet this is not recognized by foreign Governments, because our engineering schools are in no way recognized by our own Government.

The Civil Service Commissioners should endeavour to encourage the proper scientific training of the engineers they receive into the public service; and they can do so by recognizing the years spent in an engineering school as equivalent to the same number of years of the technical training that is now required. In the more important appointments, which at present require five years' technical training, the candidate would have to supplement his school course by an office course of at least two years; and this, in the opinion of eminent engineers, as quoted above, would be amply sufficient. In the case of the less important appointments, the school training is probably much better than what satisfies the Commissioners at present; but if it is thought that the special qualifications of an office-trained apprentice are essential, they can be easily secured by requiring in every case at least one year's office experience.

The Civil Service Commissioners should, before recognizing any engineering school as giving the instruction qualifying a candidate to compete for an appointment, inspect the school, and see that it is properly equipped, and has the means and teachers required to teach what it professes. For instance, in some schools there is no special instruction in architecture; and this special teaching should be required of any school that was recognized as qualifying candidates for specially architectural appointments. Similarly, in the case of mechanical engineering, some schools have not the means of teaching it properly; and

these schools should not be recognized as qualifying candidates for specially mechanical engineering appointments. A school that teaches civil engineering should be recognized as such, and only as such; and similarly, one that only teaches mechanical engineering should be recognized only as such. In the case of medical appointments, the State recognition of schools is already fully carried out, so that there can be no insuperable difficulty in doing the same in the case of the engineering appointments.

If the Civil Service Commissioners require further information as to the instruction imparted in engineering schools, it would be well for them to inspect University College, London, the City and Guilds of London Institute, and Cooper's Hill, all of which are easy of access from London. And if they require further information they had better appoint some competent Committee to inspect and report to them generally as to the training given in engineering schools, and as to whether they give a technical training that the Civil Service Commissioners would recognize as equivalent to some years spent in an office; and, if not, how the schools should modify their courses so as to give this instruction. Statements as to the nature and value of instruction, made by those interested in it and responsible for it, are not so valuable as independent testimony.

In conclusion, I would earnestly press upon the Civil Service Commissioners the very great desirability of their encouraging scientifically trained candidates to apply for appointments in the Civil Service. The application of scientific principles to engineering is the special feature of our age, and instruction in these principles, and practical training in their application, should be part of the training of every engineer; and this can only be acquired in a properly equipped school. A want of familiarity with details will surely be remedied, but a want of scientific knowledge will be a lasting cause of danger to the public.

Yours very truly,

GEORGE FRANCIS FITZ GERALD.

49

ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION

[From the *Report of the British Association for the Advancement of Science*, 1888.]

THE British Association in Bath, and especially we here in Section A, have to deplore a very great loss. We confidently anticipated profit and pleasure from the presence in this chair of one of the leading spirits of English science, Dr. Schuster. We deplore the loss and we deplore the cause of it. It is always sad when want of strength makes the independent dependent, and it is doubly sad when a life's work is thereby delayed; and to selfish humanity it is trebly sad when, as in this case, we ourselves are involved in the loss. And our loss is great. Dr. Schuster has been investigating some very important questions. He has been studying electric discharges in gases, and he has been investigating the probably allied question of the variations of terrestrial magnetism. We anticipated his matured pronouncements upon these subjects, and also the advantage of his very wide general information upon physical questions, and the benefit of his judicial mind while presiding here.

As to myself, his substitute, I cannot express how much gratified I feel at the distinguished honour done me in asking me to preside. It has been one of the ambitions of my life to be worthy of it, and I will do my best to deserve your confidence; man can do no more, and upon such a subject "the less said the soonest mended."

I suppose most former occupants of this chair have looked

over the addresses of their predecessors to see what sort of a thing was expected from them. I find that very few had the courage to deliver no address. Most have devoted themselves to broad general questions, such as the relations of mathematics to physics, or more generally of deductive to inductive science. On the other hand, several have dealt each with his own speciality. On looking back over these addresses my attention was specially arrested by the first two past presidents of this section, whose bodily presence we cannot have here. They were presidents of Section A in consecutive years. In 1874 Provost Jellett occupied this chair, and in 1875 Professor Balfour Stewart occupied it. Both have gone from us since the last meeting of this association. Each gave a characteristic address. The Provost, with the clearness and brilliancy that distinguished his great intellect, plunged through the deep and broad question surrounding the mechanism of the universe, and with impassioned earnestness claimed on behalf of science the right to prosecute its investigations until it attains, if it ever does attain, to a mechanical explanation of all things. This intrepid honesty to carry to their utmost the principles of whose truth he was convinced, the utter abhorrence of the shadow of double dealing with truth, was eminently characteristic of one whom all, but especially we of Trinity College, Dublin, will long miss as a lofty example of the highest intellectual keenness and honesty, and mourn as the truest-hearted friend, full of sympathy and Christian charity. In 1875 Professor Stewart gave us a striking example of the other class of address, in a splendid exposition of the subject he did so much to advance, namely, solar physics. He brought together from the two great storehouses of his information and speculation a brilliant store, and displayed them here for the advancement of science. Him, too, all science mourns. Though, from want of personal acquaintance, I am unequal to the task of bringing before you his many abilities and great character, you can each compose a fitting epitaph for this well-known great one of British science. In this connexion I am only expressing what

we all feel when I say how well timed was the royal bounty recently extended to his widow. At the same the niggardly recognition of science by the public is a disgrace to the enlightenment of the nineteenth century. What chancellor or general with his tens of thousands has done that for his country and mankind that Faraday, Darwin, and Pasteur have done? The "public" now are but the children of those who murdered Socrates, tolerated the persecution of Galileo, and deserted Columbus.

In a presidential address on the borderlands of the known, delivered from this chair, the great Clerk Maxwell spoke of as an undecided question whether electromagnetic phenomena are due to a direct action at a distance or are due to the action of an intervening medium. The year 1888 will be ever memorable as the year in which this great question has been experimentally decided by Hertz in Germany, and, I hope, by others in England. It has been decided in favour of the hypothesis that these actions take place by means of an intervening medium. Although there is nothing new about the question, and although most workers at it have long been practically satisfied that electromagnetic actions are due to an intervening medium, I have thought it worth while to try and explain to others who may not have considered the problem, what the problem is and how it has been solved. A presidential address such as this is not for specialists; it is for the whole section; and I would not have thought of dealing with this subject only that its immediate consequences reach to all the bounds of physical science, and are of interest to all its students.

We are all familiar with this, that when we do not know all about something there are generally a variety of explanations of what we do know. Whether there is anything of which there are in reality a variety of explanations is a deep question which some have connected with the Freedom of the Will, but which I am not concerned with here. A notable example of the possibility of a variety of explanations for us is recorded in connexion with an incident said to have occurred in the

neighbouring town of Clifton, where a remarkable meteorological phenomenon, as it appeared to an observing scientist, was explained by others as a bull's-eye lantern in the hands of Mr. Pickwick. Another kind of example is the old explanation of water rising in a pump, that "Nature abhors a vacuum," as compared with the modern one. Nowadays, when we know as little about anything, we say, "It is the property of electricity to attract." This is really little or no advance on the old form, and is merely a way of stating that we know a fact but not its explanation. There are plenty of cases still where a variety of explanations is possible. For example, we know of no *experimentum crucis* to decide whether the people I see around me are conscious or are only automata. There are other questions which have existed, but which have been experimentally decided. The most celebrated of these are the questions between the caloric and kinetic theories of heat, and between the emission and undulatory theories of light. The classical experiments by which the case has been decided in favour of the kinetic theory of heat and the undulatory theory of light are some of the most important experiments that have ever been performed. It was shown that heat disappeared whenever work appeared, and *vice versâ*, and so the caloric hypothesis was disproved; when it was shown that light was propagated more slowly in a dense medium than in a rare, the sciences of light and heat were revolutionized. Not but that most who studied the subject had given their adhesion to the true theory before it was finally decided in general estimation. In fact, Rumford's and Davy's experiments on heat, and Young's and Fresnel's experiments on light, had really decided these questions long before the erroneous views were finally abandoned.

I hope that science will not be so slow in accepting the results of experiment in respect of electromagnetism as it was in the cases of light and heat. Rowland's experiment proving an electromagnetic action between electric charges, depending on their absolute and not relative velocities, has already proved the existence of a medium relative to which the motion must take

place; but the connexion is rather metaphysical and is too indirect to attract general attention. The importance of the striking experiments was that they put the language of the wrong hypothesis out of fashion. Elementary text-books that halted between two opinions, and, after the manner of text-books, leant towards that enunciated in preceding text-books, had all perforce to give prominence to the true theory, and the whole rising generation began their researches from a firm and true standpoint. I anticipate the same results to follow Hertz's experimental demonstration of a medium by which electromagnetic actions are produced. Text-books which have gradually been invoking lines of force, in some respects to the aid of learners and in others to their bewilderment, will now fearlessly discourse of the stresses in the ether that cause electric and magnetic force. The younger generation will see clearly in electromagnetic phenomena the working of the all-pervading ether, and this will give them a firm and true standpoint for further advances.

And now I want to spend a short time in explaining to you how the question has been decided. An illustrative example may make the question itself clearer, and so lead you to understand the answer better. In colloquial language we say that balloons, hot air, etc., rise because they are light. In old times this was stated more explicitly, and therefore much more clearly. It was said that they possessed a quality called "levity." "Levity was opposed to heaviness." Heaviness made things tend downwards, levity made things tend upwards. It was a sort of action at a distance. At least it would have required such a hypothesis if it had survived until it was known that heaviness was due to the action of the earth. I expect levity would have been attributed to the direct action of heaven. It was comparatively recently in the history of mankind that the rising of hot air, flames, etc., was attributed to the air. Everybody knew that there was air, but it was not supposed that the upward motion of flames was due to it. We now know that this and the rising of balloons are due to the difference of

pressure at different levels in the air. In a similar way we have long known that there is an ether, an all-pervading medium, occupying all known space. Its existence is a necessary consequence of the undulatory theory of light. People who think a little but not much sometimes ask me, "Why do you believe in the ether? What's the good of it?" I ask them, "What becomes of light for the eight minutes after it has left the Sun and before it reaches the Earth?" When they consider that they observe how necessary the ether is. If light took no time to come from the Sun there would be no need of the ether. That it is a vibratory phenomenon, that it is affected by matter it acts through—these could be explained by action at a distance very well. The phenomena of interference would, however, require such complicated and curious laws of action at a distance as practically to put such a hypothesis out of court or else be purely mathematical expressions for wave-propagation. In fact, anything except propagation in time is explicable by action at a distance. It is the same in the case of electromagnetic actions. There were two hypotheses as to the causes of electromagnetic actions. One attributed electric attraction to a property of a thing called electricity to attract at a distance, the other attributed it to a pull exerted by means of the ether somewhat in the way that air pushes balloons up. We do not know what the structure of the ether is by means of which it can pull, but neither do we know what the structure of a piece of indiarubber is by means of which it can pull; and we might as well ignore the indiarubber, though we know a lot about the laws of its action, because we do not know its structure, as ignore the ether because we do not know its structure.

Anyway, what was wanted was an experiment to decide between the hypotheses of direct action at a distance and of action by means of a medium. At the time that Clerk Maxwell delivered his address no experiment was known that could decide between the two hypotheses. Specific inductive capacity, the action of intervening matter, the delay

in telegraphing, the time propagation of electromagnetic actions by means of conducting material—these were known, but he knew that they could be explained by means of action at a distance, and had been so explained. Waves in a conductor do not necessarily postulate action through a medium such as the ether. When we are dealing with a conductor and a thing called electricity running over its surface, we are of course postulating a medium on or in the conductor, but not outside it, which is the special point at issue. Clerk Maxwell *believed* that just as the same air that transmits sound is able by differences of pressure—*i. e.* by means of its energy per unit volume—to move bodies immersed in it, so the same ether that transmits light causes electrified bodies to move by means of its energy per unit volume. He believed this; but there was no experiment then known to decide between this hypothesis and that of direct action at a distance. As I have endeavoured to impress upon you, no *experimentum crucis* between the hypotheses is possible except an experiment proving propagation in time, either directly, or indirectly by an experiment exhibiting phenomena like those of the interference of light. A theorist may speak of propagation of actions in time without talking of a medium. This is all very well in mathematical formulæ; but as in the case of light, we must consider what becomes of it after if has left the Sun and before it reaches the Earth, so every hypothesis assuming action in time really postulates a medium whether we talk about it or not. There are some difficulties surrounding the complete interpretation of some of Hertz's experiments. The conditions are complicated, but I confidently expect that they will lead to a decision on most of the outstanding questions on the theory of electromagnetic action. However, there is no doubt that he has observed the interference of electromagnetic waves quite analogous to those of light, and that he has proved that electromagnetic actions are propagated in air with the velocity of light. By a beautiful device Hertz has produced rapidly alternating currents of such frequency that their wave-length is only about two metres. I may pause

for a minute to call your attention to what that means. These waves are propagated three hundred thousand kilometres in a second. If they vibrated three hundred thousand times a second the waves would be each a kilometre long. This rate of vibration is much higher than the highest audible note, and yet the waves are much too long to be manageable. We want a vibration about a thousand times as fast again, with waves about a metre long. Hertz produced such vibrations, vibrating more than a hundred million times a second. That is, there are as many vibrations in one second as there are seconds—in a day? No, far more. In a week? No, more even than that. The pendulum of a clock ticking seconds would have to vibrate for four months before it would vibrate as often as one of Hertz's vibrators vibrates in one second. And how did he detect the vibrations and their interference? He could not see them; they are much too slow for that; they should go about a million times as fast again to be visible. He could not hear them; they are much too quick for that. If they went a million times more slowly they would be well heard. He made use of the principle of resonance. You all understand how by a succession of well-timed small impulses a large vibration may be set up. It explains many things, from speech to spectrum analysis. It is related that a former Marquess of Waterford used the principle to overturn lamp-posts; his ambition soared above knocker-wrenching. So that it is a principle known to others besides scientific men. Hertz constructed a circuit whose period of vibration for electric currents was the same as that of his generating vibrator, and he was able to see sparks, due to the induced vibration, leaping across a small air-space in this resonant circuit. The well-timed electrical impulses broke down the air-resistance just as those of my Lord of Waterford broke down the lamp-post.

The combination of a vibrating generating circuit with a resonant receiving circuit is one that I spoke of, at the meeting of the British Association at Southport,* as one by which

[* In 1883; No. 27, *supra*.]

this very question might be studied. At the time I did not see any feasible way of detecting the induced resonance; I did not anticipate that it could produce sparks. By its means, however, Hertz has been able to observe the interference between waves incident on a wall and the reflected waves. He placed his generating vibrator several wave-lengths away from a wall, and placed the receiving resonant circuit between the generator and the wall; and in this air-space he was able to observe that at some points there were hardly any induced sparks, but at other and greater distances from his generator they reappeared, to disappear again in regular succession at equal intervals, between his generator and the wall. It is exactly the same phenomenon as what are known as Lloyd's bands in optics, which are due to the interference between a direct and a reflected wave. It follows hence that, just as Young's and Fresnel's researches on the interference of light prove the undulatory theory of optics, so Hertz's experiment proves the ethereal theory of electromagnetism. It is a splendid result. Henceforth I hope no learner will fail to be impressed with the theory—hypothesis no longer—that electromagnetic actions are due to a medium pervading all known space, and that it is the same medium as the one by which light is propagated; that non-conductors can, and probably always do, as Professor Poynting has taught us, transmit eleectromagnetic energy. By means of variable currents energy *is* propagated into space with the velocity of light. The rotation of the earth is being slowly stopped by the diurnal rotation of its magnetic poles. This seems a hopeful direction in which to look for an explanation of the secular precession of terrestrial magnetism. It is quite different from Edlund's curious hypothesis that free space is a perfect conductor. If that were true, there would be a pair of great antipoles outside the air, and terrestrial magnetism would not be much like what it is, and I think the earth would have stopped rotating long ago. With alternating currents we *do* propagate energy through non-conductors. It seems almost as if our future telegraph cables would be pipes.

Just as the long sound-waves in speaking tubes go round corners, so these electromagnetic waves go round corners if they are not too sharp. Professor Lodge will probably have something to tell us on this point in connexion with lightning conductors. The silvered-glass bars used by surgeons to conduct light are exactly what I am describing. They are of glass, a non-conducting, and therefore transparent, bar surrounded by a conducting, and therefore opaque, silver sheath; and they transmit the rapidly alternating currents we call light.

There would not be the same difficulty in utilising the energy of these electromagnetic waves as in utilising radiant heat. Having all the vibrations of the same period we might utilise Hertz's resonating circuits; and in any case the second law of thermodynamics would not trouble us when we could practically attain to the absolute zero of these, as compared with heat, long period vibrations.

We seem to be approaching a theory as to the structure of the ether. There are difficulties from diffusion in the simple theory that it is a fluid full of motion, a sort of vortex sponge. There were similar difficulties in the wave-theory of light owing to wave-propagation round corners; and there is as great a difficulty in the jelly theory of the ether arising from the freedom of motion of matter through it. It may be found that there is diffusion; or it may be found that there are polarized distributions of fluid kinetic energy which are not unstable when the surfaces are fixed,—more than one such is known. Osborne Reynolds has pointed out another, though in my opinion less hopeful, direction in which to look for a theory of the ether. Hard particles are abominations. Perhaps the impenetrability of a vortex would suffice. Oliver Lodge speaks confidently of a sort of chemical union of two opposite kinds of elements forming the ether. The opposite side of a vortex ring might perchance suit; or maybe the ether, after all, is but an atmosphere of some infra-hydrogen element: these two latter hypotheses may both come to the same thing. Anyway we are learning daily what sort of properties the ether must have. It must be the means

of propagation of light; it must be the means by which electric and magnetic forces exist; it should explain chemical actions and, if possible, gravity.

On the vortex-sponge theory of the ether there is no real difficulty, by reason of complexity, why it should not explain chemical actions. In fact, there is every reason to expect that very much more complex actions would take place at distances comparable with the size of the vortices, than at the distances at which we study the simple phenomena of electromagnetism. Indeed, if vortices can make a small piece of a strong elastic solid, we can make watches and build steam engines and any amount of complex machinery, so that complexity can be no essential difficulty. Similarly, the instantaneous propagation of gravity, if it exists, is not an essential difficulty, for vortices each occupy all space, and they act on one another simultaneously everywhere. The theory that material atoms are simple vortex rings in a perfect liquid otherwise unmoving is insufficient, but with the innumerable possibilities of fluid motion it seems almost impossible but that an explanation of the properties of the universe will be found in this conception. Anything purporting to be an explanation founded on such ideas as "an inherent property of matter to attract," or building up big elastic solids out of little ones, is not of the nature of an ultimate explanation at all; it can only be a temporary stopping place. There are metaphysical grounds, too, for reducing matter to motion, and potential to kinetic energy.

These ideas are not new, but it is well to enunciate them from time to time, and a presidential address in Section A is a fitting time. Besides all this it has become the fashion to indulge in quaint cosmical theories, and to dilate upon them before learned societies and in learned journals. I would suggest, as one who has been bogged in this quagmire, that a successor in this chair might well devote himself to a review of the cosmical theories propounded within the last few years. The opportunities for piquant criticism would be splendid.

Returning to the sure ground of experimental research, let

us for a moment contemplate what is betokened by this theory that in electromagnetic engines we are using as our mechanism the ether, the medium that fills all known space. It was a great step in human progress when man learnt to make material machines, when he used the elasticity of his bow and the rigidity of his arrow to provide food and defeat his enemies. It was a great advance when he learnt to use the chemical action of fire; when he learnt to use water to float his boats and air to drive them; when he used artificial selection to provide himself with food and domestic animals. For two hundred years he has made heat his slave to drive his machinery. Fire, water, earth, and air have long been his slaves; but it is only within the last few years that man has won the battle lost by the giants of old, has snatched the thunderbolt from Jove himself and enslaved the all-pervading ether.

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RUDOLPH JULIUS EMMANUEL CLAUSIUS

[From the Obituary Notices, *Proceedings of the Royal Society*, Vol. 48, 1888.]

RUDOLF JULIUS EMMANUEL CLAUSIUS was born on the 2nd January, 1822, in Cöslin, in Pomerania. He was the sixth son of the Rev. C. E. G. Clausius, D.D., Councillor of the Royal Government School Board, and later, Superintendent in Ueckermünde.

After the completion of his studies in the Gymnasium in Stettin, he attended the University of Berlin from 1840 to 1844. In the Easter of 1844 he passed his examination "pro facultate docendi," and then finished his year of probation at the Frederic-Werder Gymnasium. Here he taught the higher classes mathematics and physics. In the autumn of 1846 he entered Boeck's Royal Seminary for higher students. On the 15th July, 1848, he took his degree in Halle "eximia cum laude" (subject of dissertation—"De iis Atmosphæræ Particulis quibus Lumen reflectitur"). On the 25th September, 1850, he was invited to be Professor of Physics in the Royal Artillery and Engineering School at Berlin. On the 18th December he delivered his inaugural lecture as *docent* at the University of Berlin ("De Motu Corporum rotantium in Aëre resistente"). On the 29th August, 1855, he was called to be Ordinary Professor in the Polytechnicum in Zurich, and also at the same time in the University of Zurich. In 1867 he was appointed Professor in the University of Würzburg, and in 1869 he went to Bonn, where he fulfilled his duties till the day of his death, the 24th August, 1888.

While at Zurich he married, on the 13th November, 1859, Adelheid Rimpam, of Brunswick. They had six children, of whom two daughters and two sons are alive. His wife died in 1875, and he married again, in 1886, Sophie Sack, of Essen, by whom he had one son.

His brother, Herr Robert Clausius, thus writes of the character of this great man:—

"I had often the opportunity of admiring the rare energy and clearness with which, in a small study and with limited means, he untiringly pursued his great scientific aims. A chief characteristic was his sincerity and fidelity. Every kind of exaggeration was opposed to his nature. Even as a youth all intimate with him learnt to esteem his reliability and truthfulness. In the Gymnasium and in all circumstances of his later life the greatest confidence and trust were placed in him. His judgment, which was guided by the strongest feeling of rectitude, was highly valued. Another important trait was his unbending and firm faithfulness to duty, to which he was true in all affairs of life up to his death. Even on his last bed of sickness he held an examination. He was the best and most affectionate of fathers, fully entering into the joys of his children. He himself supervised the schoolwork of his children. He was simple and natural in his intercourse and possessed of a rare modesty. He was never tired of sacrificing himself in cases of necessity, and though immersed in abstract studies he always kept a warm heart for everything human. He was a great, noble-minded, good man, whom all who knew more intimately have loved and esteemed, not only on account of his scientific celebrity but especially on account of his noble, manly qualities.

"His burning patriotism did not permit him to stay idle at home during the war of 1870–1. He undertook the leadership of an ambulance corps, which he formed of Bonn students. At the great battles of Vionville and Gravelotte he helped to carry the wounded from the battle and to lessen their sufferings. For his services in this campaign he received the Iron Cross. A contusion in his leg which he received on the field of battle

caused him great pain for many years and often necessitated his driving to his lectures. On the doctor's advice, at the age of 56 he learnt to ride, and became an excellent horseman. Riding proved very beneficial to his health."

One of his sons thus writes of him:—

"The principal trait in my father's character was, without doubt, the splendid truthfulness of his nature. In his deeds and words he never could tolerate anything ambiguous, and particularly as regards himself he cherished no self-deception. For that reason he never suffered from the discovery of the motives of his actions; from thence sprang his thoroughly noble nature as well as his great modesty and the delight which he always felt, and was never too proud to express, at the recognition of his work; from thence his dislike to all smattering and superficiality in which he suspected some untruthfulness. Another remarkable characteristic was the uncommon one of seeing only the best side of his neighbours. He hardly noticed their faults, and when he did he had not the least inclination to cheap mockery. The general impression which he received of people was formed from the more or less strong development of their good qualities and was little dimmed by the presence of this or that defect. His hearty, pleasant, always thoroughly genuine address towards everyone was the result of this trait. Only when he discovered untruthfulness did he take a deep aversion."

Clausius received the following offers of posts:—

In 1858 to the Polytechnicon of Carlsruhe; in 1862 to the Polytechnicum of Brunswick; in 1866 to Vienna; in 1868 to Munich; in 1871 to Strasburg; in 1883 to Göttingen.

He received the following orders and titles:—

When Professor in Würzburg he was appointed Court Councillor; in 1868, as Professor in Bonn, he was appointed Privy Councillor; in 1870 he received the Huygens Medal; in 1871 the Iron Cross; in 1873 the Order of the Crown, 3rd Class; in 1879 the Order of the Red Eagle, 3rd Class, and the Copley Medal; in 1881 he was named Officer of the Legion of Honour;

in 1882 he was appointed Doctor of Medicine *honoris causâ* in Würzburg; in 1883 he received the Poncelet prize; in 1884 the Order of the Prussian Crown, 2nd Class; in 1885 the Bavarian Maximilian Order. During 1884–5 he was Rector of the University of Bonn, and for six months was Curator of this University. He took part in the academic education of Prince William, afterwards Emperor of Germany. In 1887 he was invited to be one of the Curators of the New Imperial Physical and Technical Institution. In 1888, at the Investiture of the Order for Art and Science, he was appointed Hereditary Knight.

He received the following honours from Learned Societies:—

In 1857 Hon. Mem. Harlem; in 1859 Hon. Mem. of the Engineers of Scotland and Corresp. Mem. Erlangen; in 1865 Corresp. Mem. of the Institute of France; in 1866 Hon. Mem. Frankfurt-am-Main, and Hon. Mem. Dublin and Corresp. Mem. Göttingen; in 1868 Royal Soc. Lond.; in 1869 Hon. Mem. Nat. His. Soc. Zurich; in 1871 Elected Mem. Zurich; in 1872 Elected Mem. Pest; in 1873 Corresp. Mem. Bologna and Elected Hon. Mem. Boston; in 1875 Hon. Mem. Civil Engin. Lond., Elected Corresp. Mem. Vienna, and Mem. Brussels; in 1876 Corresp. Mem. Berne, Society of Arts Geneva, and Hon. Mem. Nat. Hist. Soc. Basel; in 1877 Elected Mem. Göttingen; in 1878 Elected Mem. Stockholm, Elected Mem. Naples and St. Petersburg; in 1879 Mem. Halle Natural Hist. Soc. and Elected Mem. Ling.; in 1882 Corresp. Mem. Milan, Corresp. Mem. Turin, and Hon. Mem. Mech. Eng. New York; in 1883 Elected Mem. Washington, Hon. Mem. Erlangen, and Foundation Mem. Internat. Soc. of Electricians; in 1884 Corresp. Mem. Cherbourg and Corresp. Mem. Lucca, Hon. Mem. Manchester, Mem. Amsterdam; in 1887 Hon. Mem. Brunswick, Hon. Mem. Hamburg, Ordinary Mem. Upsala; in 1888 Mem. Edin.

In his scientific work Clausius investigated the general mechanism of Nature rather than particular applications of the principles he discovered; he was constructively synthetical rather than analytical. It is remarkable how he was led by

dim previsions, as when his theory of gases influenced his views on heat when his gas theory was yet quite imperfect; his greatest work is grouped round his insight into molecular structure. The other great branch of his work is connected with electromagnetic theory. As in the theory of heat he worked from the theory of matter to the theory of the steam engine, so in electromagnetism he worked from the theory of electromagnetic actions to the theory of its industrial application to dynamos.

Clausius' first publications were concerned with the action of atmospheric dust on sunlight. This seems to have directed his thoughts to molecular physics, which was indirectly the foundation of his greatest work. Thermodynamics was the region he explored, and his exploration was guided by his insight into molecular physics. When Clausius was beginning his independent activity the investigations of Rumford, Davy, Mayer, Joule, and Helmholtz had conclusively shown that heat could be produced from work, while the thermodynamic speculations of Carnot, founded upon the assumed indestructability of caloric, were receiving every day additional confirmation. There was an obvious difficulty here of which Carnot himself was doubtless aware. Of this difficulty, in 1849, Sir William Thomson writes, that if we abandon Carnot's fundamental axiom "we meet with innumerable other difficulties insuperable without further investigation and an entire reconstruction of the theory of heat from its foundations. It is, in reality, to experiment that we must look, either for a verification of Carnot's axiom and an explanation of the difficulty we have been considering, or for an entirely new basis of the theory of heat."

It was at this juncture that Clausius, without waiting for additional experiments, read, in the Berlin Academy on the 18th February, 1850, his paper, "Ueber die bewegende Kraft der Wärme, und die Gesetze, welche sich daraus für die Wärmelehre selbst ableiten lassen."

Carnot has assumed that a heat engine gave out the same

heat at the lower temperature as it took in at the higher, and founded his theory on this assumption and upon the impossibility of perpetual motion. Clausius, in the first place, emphasised that the heat given out must be less than the heat taken in by an amount equivalent to the work done, that this was required by the First Law of Thermodynamics, the equivalence of Heat and Work. Thus modified, Carnot's theorems could no longer rely for their proof on the impossibility of perpetual motion, and it was Clausius' great discovery to found Thermodynamics upon the New Second Law of Thermodynamics, "That heat tends to flow of itself from hot to cold bodies." On these foundations Clausius raised again the theory of Thermodynamics, and thenceforward there was no serious doubt as to its security. Several different ways of stating the Second Law of Thermodynamics have been advocated, and objections have been raised to each of them. Of these things Clerk Maxwell writes that Clausius "first stated the principle of Carnot in a manner consistent with the true theory of heat." Of the varieties of statement he writes:—"By comparing together these statements the student will be able to make himself master of the facts which they embody, an acquisition which will be of much greater importance to him than any form of words on which a demonstration may be more or less compactly constructed."

There can be no doubt that Clausius was the first to throw a clear light upon the then dark and doubtful foundations of Carnot's theorem. Sir William Thomson writes of this in 1851, "the merit of first establishing the proposition upon correct principles is entirely due to Clausius."

As Professor Willard Gibbs says, "Rankine was attacking the problem in his own way with one of those marvellous creations of the imagination of which it is so difficult to estimate the precise value." The question of the amount of mechanical effect to be derived from heat, he further says, "was completely answered, on its theoretic side, in the memoir of Clausius, and the science of thermodynamics came into exist-

ence." "It might be said, at any time, since the publication of that memoir, that the foundations of the science were secure, its definitions clear, and its boundaries distinct." To Clausius then be the honour of making a science of Thermodynamics.

Clausius' subsequent work in this line consists essentially in working out the results of the law he discovered, and in investigating its foundations on general dynamical principles applied to molecular physics. In working out the results of his law he explored in two directions. He applied his discovery to work out the theory of the steam engine, and of many known phenomena, and also to discover properties of matter revealed by his analysis. This latter line is contiguous with his exploration of the dynamical foundations of the theory of heat. His analysis revealed the existence of entropy as a property of matter, a property for which mankind has no sense, such as exists for feeling temperature, and which consequently escapes attention, and is most difficult of apprehension; so difficult, indeed, that although fundamentally as important as temperature in the theory of steam engines, its existence is ignored by all except the very foremost amongst those who study the working of steam engines. Clausius has left little to be done in the theory of heat engines except to work out, in the lines he has laid down, the details that experiments may prove to be most important. Clausius applied his theory to investigate the laws of specific and latent heat, of saturated steam, of the relations of heat and electricity in conductors, in thermopiles, and in electrolytes, and to the laws of radiant heat. He showed that radiant heat was no exception to the law that heat flows of itself from hot to cold bodies, and so proved the futility of the ingenious suggestion that the death of the universe by the degradation of energy might be avoided by the reconcentration of heat radiations by reflection from the confines of the ether. He showed that the radiating power of black bodies in various media was proportional to the square of the refractive index of the medium, which involves a corresponding law of radiation of electromagnetic energy.

In all these applications his attention was constantly directed to the underlying molecular motions which explained the phenomena on dynamical principles. It was in this connexion that he investigated the dynamical foundations of the Second Law of Thermodynamics and the molecular theory of gases and of electrolysis.

In quite an early investigation he had been dominated by the conception that heat in a body can be considered as separated into two parts, one of the kinetic energy of atoms, and the other the potential energy of forces between atoms. His later investigations were elaborations of these conceptions by the application of statistical methods, and by mathematical analysis of the highest order. He showed that the heat in a body could be expressed as the product of two factors, one proportional to the mean kinetic energy of the atoms, and the other depending on the mass, velocity, and period of their motions. These factors may be identified with temperature and entropy, and so furnish a dynamical basis for the theory of heat. Involved in these investigations was the Theorem of the Virial, which is so important in the dynamics of stationary motion. His theory is in accordance with much that we know, though it neglects radiation, and forces between molecules depending on their motions and positions, which may be systematically different before and after collision. With the ether among the molecules it is almost impossible but that some such forces exist, while the success of dynamical theories that neglect them seems to show that their effect cannot be very great.

While Clausius was elaborating these general results, he attacked the simpler case of the molecular theory of gases. That the properties of gases were in some way due to the motions of their molecules was a hypothesis as old at least as Bernoulli, but it was Clausius who raised it to the rank of a theory, and he has been described by Clerk Maxwell as the principal founder of the science. He showed how Boyle's and Dalton's Laws followed from the theory, and how they were

approximate; he proved the existence of intramolecular energy, and the necessity for Avogadro's Law, and for the equality of mean energy of translation; and, insisting on the necessity for two atoms in a molecule, hastened the advent of the change in atomic weights which chemists were adopting; he investigated the length of the mean free path of a molecule, and the rates of diffusion and conductivity of heat in gases.

In connexion with the molecular theory of matter, Clausius had, as early as 1851, investigated some of the laws of evaporation, and had shown that the law of corresponding temperatures that Groshans modified from Dalton involved the law that at any given pressure the latent heat of vaporisation per unit volume of many substances was the same. After Van der Waals' memorable paper on the continuity of the liquid and gaseous states, Clausius published an elaborate investigation of this subject, in which he developed the application of a rather complicated formula, and showed that it represented the experiments throughout an enormous range with wonderful accuracy. There is a measurable departure from the law in calculating the compressibility of the liquids. To facilitate the application of this formula, Clausius invented and calculated the values of a special transcendental.

Electricity and magnetism attracted Clausius' attention from time to time, at first in connexion with heat and molecular physics, and afterwards with reference to the theory of electrokinetic actions. In 1858, he developed the theory that the molecules in electrolytes are continually interchanging atoms, and that the effect of electric force is to direct the interchange and not to cause it. It seems, however, possible that as synchronous systems near the solar system might break it up, while asynchronous ones might not, so a polarization of the atomic motions in a liquid might result in a proportionate breaking up of the molecules which before this introduced regularity were stable. In reply to Hittorf's objection that gases should obey Ohm's Law, Clausius answered that there were too few molecules. This can hardly be considered conclusive in presence of electro-

lytic conduction in very dilute aqueous solutions. His theory, however, explains almost all the known facts of electrolysis, and has been extended by others to explain many other phenomena with most remarkable success. He investigated electric osmosis, and hints that it is produced by electric forces due to charges over the surfaces of the pores in the diaphragm. He remarked that the resistances of pure metals is proportional to their absolute temperature.

His electrokinetic theory, founded on a theory of action at a distance between electrical elements moving in conductors, led to the conclusion that the action between the elements must depend on their absolute velocity and not on their relative velocity. This practically postulates a medium with reference to which the elements move, and by which the actions are propagated.

With the great development of electrodynamics as a machine for applying energy to do man's work, Clausius repeated his exploration of the theory of the steam engine, that great machine for applying heat energy to do work, and investigated on broad principles the the theory of dynamos. Some of his work in this direction is superseded by the rapid development of the science and its applications, but his insight into the problem is evidenced by his having been one of the first to notice hysteresis, which he describes as the forces resisting magnetization being like friction.

It is to be regretted that, in his electromagnetic theory, Clausius was led by the algebraic methods of Weber, rather than by the geometrical insight of Faraday, or by some mechanical theory, such as directed his steps in thermodynamics. If his constructive genius had been here well directed, there might now exist a satisfactory theory of electromagnetic action; he might have founded the theory of ether as well as of gas; he had genius enough to do it.

Though not himself an experimentalist, he valued and was eminently able to criticise and use the results of experiment. He had that clear grasp of natural phenomena which leads to

a right interpretation of them, and that concrete practical conception of them that leads to a continual reference back of the interpretation to experimental numerical verification. He was a noble example of the spirit that devotes itself to directly benefiting mankind, and that does not waste time on petty elaborations of pretty problems. He was in the highest sense practical, his work is eternal, and his memory will live as long as mankind reveres its benefactors.

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NOTE ON THE ORIGINATION OF TURBULENT MOTION IN VISCOUS LIQUIDS

[From the *Scientific Proceedings of the Royal Dublin Society.* Read January 9, 1889.]

IN a viscous liquid moving near a fixed surface the motion is laminar if the rate of change of motion with distance from surface is not too great. Under these circumstances a small deviation from laminar motion is gradually destroyed by the viscosity. If we consider the forces acting on any wave supposed superadded to the laminar motion, we see that the pressure, on what may be called the windward side of the wave, is greater than on the lee side, on account of the viscosity of the liquid; and if the change of velocity of the liquid with distance from surface be great enough, the difference of pressure may be great enough to cause the wave to become greater and greater notwithstanding viscosity, just as the wind causes waves in the sea; and the waves will ultimately break, which corresponds to a production of turbulent motion.* If the waves originally produced be of short wave-length, they would be more affected by the viscosity stopping them than larger ones, and so only turbulency of a definite size would be produced. This theory seems to account for most of Professor Osborne Reynolds' observations.

[* The mathematical theory of this instability is investigated by Lord Rayleigh, *Proc. Math. Soc.* 1880, 1887, 1895, reprinted in his Scientific Papers.]

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ON THE DIMENSIONS OF ELECTROMAGNETIC UNITS

[From the *Philosophical Magazine* for April, 1889. Read before the Physical Society on February 23, 1889.]

SOME attention has lately been called to the question of the dimensions, of electromagnetic units, but the following obvious suggestions seems to have escaped notice.

The electrostatic system of units may be described as one in which electric inductive capacity is *assumed* to have zero dimensions and the electromagnetic system as one in which the magnetic inductive capacity is assumed to have zero dimensions. Now if we take a system, in which the dimensions of both these quantities are the same, and of the dimensions of a slowness, *i.e.* the inverse of a velocity $\left[\frac{T}{L}\right]$, the two systems become identical as regards dimensions, and differ only by a numerical coefficient just as centimetres and kilometres do. There seems a naturalness in this result that justifies the assumption that these inductive capacities are really of the nature of a slowness. It seems possible that they are related to the reciprocal of the square root of the mean energy of turbulence of the ether.

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ON AN ELECTROMAGNETIC INTERPRETATION OF TURBULENT LIQUID MOTION

[From *Nature*, *May* 9, 1889.]

THE equations of rate of increase of momentum F, components X, Y, Z, per unit volume of a liquid (density = 1) are

$$X = \frac{du}{dt} + u\frac{du}{dx} + v\frac{du}{dy} + w\frac{du}{dz} + \frac{dp}{dx},$$

and corresponding ones for Y and Z. If the liquid be incompressible,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Multiplying this by u, and adding to X, we get

$$X = \frac{du}{dt} + \frac{d}{dx}(u^2) + \frac{d}{dy}(uv) + \frac{d}{dz}(uw) + \frac{dp}{dx},$$

and similar expressions for Y and Z.

It is at once obvious that these could be written down at once, for u^2, uv, and uw are the momenta per second carried into the element through its faces as in the kinetic theory of gases; and it is evident that the equation is the same as for a strained solid where these are the superficial normal and tangential forces.

If we now take the average values of these throughout any small region, and write for the average of $\frac{du^2}{dx} = \overline{\frac{du^2}{dx}}$, we have

$$\bar{X} = \overline{\frac{du}{dt}} + \overline{\frac{du^2}{dx}} + \overline{\frac{duv}{dy}} + \overline{\frac{duw}{dz}} + \frac{dp}{dx}.$$

Comparing this with Maxwell's stress in the ether we must take* ("Electricity and Magnetism," vol. i, § 105)—

$$2\overline{\frac{du^2}{dx}} = \frac{d}{dx}\left(\frac{d\psi}{dx}\right)^2 - \frac{d}{dx}(Q^2 + R^2 - I^2) - \frac{d}{dx}(\beta^2 + \gamma^2 - a^2),$$

$$\overline{\frac{duv}{dy}} = \frac{d}{dy}\left(\frac{d\psi}{dx}\frac{d\psi}{dy}\right) + \frac{d}{dy}PQ + \frac{d}{dy}a\beta,$$

$$\overline{\frac{duw}{dz}} = \frac{d}{dz}\left(\frac{d\psi}{dx}\frac{d\psi}{dz}\right) + \frac{d}{dz}PR + \frac{d}{dz}a\gamma;$$

adding these together, and comparing with the equation for X, we get

$$\overline{X} - \overline{\frac{du}{dt}} - \frac{dp}{dx}$$

$$= \frac{1}{2}\frac{d}{dx}\left\{\left(\frac{d\psi}{dx}\right)^2 + \left(\frac{d\psi}{dy}\right)^2 + \left(\frac{d\psi}{dz}\right)^2\right\} + \frac{d\psi}{dx}\left\{\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2}\right\}$$

$$+ P\left(\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}\right) - Q\left(\frac{dQ}{dx} - \frac{dP}{dy}\right) + R\left(\frac{dP}{dz} - \frac{dR}{dx}\right)$$

$$+ a\left(\frac{da}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz}\right) - \beta\left(\frac{d\beta}{dx} - \frac{da}{dy}\right) + \gamma\left(\frac{da}{dz} - \frac{d\gamma}{dx}\right),$$

and two similar equations involving Y and Z.

Now, assuming

$$-p = \frac{1}{2}\left\{\left(\frac{d\psi}{dx}\right)^2 + \left(\frac{d\psi}{dy}\right)^2 + \left(\frac{d\psi}{dz}\right)^2\right\},$$

and

$$\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2},$$

$$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = e,$$

$$\frac{da}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = m,$$

[* This seems to be an identification of Maxwell's stress-terms with the corresponding ones in $\overline{X}$, ψ being introduced only to account for p.]

we get

$$X - \frac{\overline{du}}{dt} = Pe + am - Q\left(\frac{dQ}{dx} - \frac{dP}{dy}\right) + R\left(\frac{dP}{dz} - \frac{dR}{dx}\right) - \beta\left(\frac{d\beta}{dx} - \frac{d\alpha}{dy}\right) + \gamma\left(\frac{d\alpha}{dz} - \frac{d\gamma}{dx}\right),$$

and two other similar equations.

If we now *assume* as the relations connecting P, Q, R, and α, β, γ, with a velocity V,

$$V\left(\frac{dR}{dy} - \frac{dQ}{dz}\right) = \frac{d\alpha}{dt}, \text{ and } V\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right) = -\frac{dP}{dt},$$

and the corresponding others, we get

$$X - \frac{\overline{du}}{dt} = Pe + am - \frac{1}{V}\frac{d}{dt}(Q\gamma - R\beta),$$

which, if we suppose the motion steady, so that $\frac{\overline{du}}{dt} = 0$, gives exactly the right expression* for the mechanical force due to an electric charge, e, and a magnetic charge, m, and an electric current with components

$$\frac{dP}{dt}, \frac{dQ}{dt}, \frac{dR}{dt}.$$

In order to justify the assumed relation between α, β, γ and P, Q, R, compare with Sir William Thomson's equations of propagation of disturbance in a turbulent liquid (*Phil. Mag.*, October 1887, p. 342).

In his case everything is a function of y only, and in that case my equations become, when there is no mechanical force,

$$-\frac{\overline{du}}{dt} = \frac{1}{V}\frac{d}{dt}(R\beta - Q\gamma) + P\frac{dQ}{dy} + \alpha\frac{d\beta}{dy} = \frac{d}{dy}(PQ + \alpha\beta).\dagger$$

[* This is *not* the correct expression; in fact the electric and magnetic stresses of Maxwell *acting together* do not seem to give it on his own hypothesis; *cf.* his Treatise, § 643. For their adaptation to a molecular theory, see Larmor, *Phil. Trans.* A 1897, p. 251.]

[† This has been considerably altered.]

If we now assume that during the passage of a disturbance of the kind, $Q = \beta =$ constant, and they must be so if

$$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = 0, \quad \frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0,$$

we get

$$-\frac{\overline{du}}{dt} = \frac{1}{V}\frac{d}{dt}(RB - Qy),$$

and consequently

$$\frac{d}{dt}(R\beta - Q\gamma) = V\frac{d}{dy}(PQ + \alpha\beta).$$

Comparing this with Sir William Thomson's equation (34) we see that

$$f = -\frac{R\beta - Q\gamma}{V}, \quad xzav(uv) = (PQ + \alpha\beta).$$

Similarly, by calculating

$$\frac{d}{dt}(PQ + \alpha\beta) = Q\frac{dP}{dt} + \beta\frac{d\alpha}{dt}$$

$$= -QV\frac{d\gamma}{dy} + \beta V\frac{dR}{dy} = +V\frac{d}{dy}(R\beta - Q\gamma),$$

we reproduce Sir William Thomson's equation (49), if $V^2 = \frac{2}{3}R^2$, *i.e.* be $\frac{2}{3}$ of the average square of velocity of turbulency. This V is the velocity of propagation of the disturbance.

If we wish to identify this laminar motion of Sir William Thomson's with a simple wave-propagation such as light consists of, we must take $Q = \beta =$ constant, and then the two equations are satisfied by*

$$\frac{dP}{dt} = -V\frac{d\gamma}{dy} \quad \text{and} \quad \frac{d\gamma}{dt} = -V\frac{dP}{dy},$$

and

$$\frac{dR}{dt} = V\frac{d\alpha}{dy} \quad \text{and} \quad \frac{d\alpha}{dt} = V\frac{dR}{dy},$$

no matter what Q and β are, and these are, of course, Maxwell's equations. There is nothing to settle which is the electric and which the magnetic disturbance, nor even which, f or $xzav(uv)$, is proportional to $R\beta - Q\gamma$, and which to $PQ + \alpha\beta$; but the

[* The upper half of the page seems to add nothing to this, which is all in the *assumed* relations.]

consideration that electric currents and electrification are possible while magnetic currents do not exist, will probably decide a question of this kind. In Maxwell's simplest wave, P and γ only exist, and in this case, as I have assumed above, $xzav(uv)$ would correspond to electric, and f to magnetic disturbance. In Sir William Thomson's representation, $xzav(uv)$ is of the nature of a twist, and f of a flow, contrary to the usual notion that magnetic force is twisty. However, a flow cannot take place outwards continuously from a body, so that there seems a reasonableness in likening electrification to a twist. The fact that magnetism in matter rotates the plane of polarization sometimes to one side and sometimes to the other does not prove conclusively that it is a rotation: a flow might confer that property on matter.

In order to include the general case of a variable state, an interpretation of X, Y, Z is required. Where no matter is present, we must assume*

$$-\mathrm{X} = \frac{dp_1}{dx} + \frac{1}{2}\frac{d}{dx}(\mathrm{P}^2 + \mathrm{Q}^2 + \mathrm{R}^2 + \alpha^2 + \beta^2 + \gamma^2), \text{ etc.};$$

while in the steady state

$$p_1 = -\tfrac{1}{2}(\mathrm{P}^2 + \mathrm{Q}^2 + \mathrm{R}^2 + \alpha^2 + \beta^2 + \gamma^2).$$

When the state is not steady, we have, if

$$\frac{d\mathrm{P}}{dx} + \frac{d\mathrm{Q}}{dy} + \frac{d\mathrm{R}}{dz} = e, \quad \frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = m,$$

$$\mathrm{X} - \frac{\overline{du}}{dt} = \mathrm{P}e + \alpha m - \frac{d}{dt}\frac{(\mathrm{Q}\gamma - \mathrm{R}\beta)}{\mathrm{V}}, \text{ etc.}$$

We must now assume that p_1 has no longer the above value, as X, Y, Z do not vanish; but by differentiating the first of these with respect to x, the second with respect to y, and the third with respect to z, we get

$$\frac{d\mathrm{X}}{dx} + \frac{d\mathrm{Y}}{dy} + \frac{d\mathrm{Z}}{dz} - \frac{d}{dt}\left(\frac{d\bar{u}}{dx} + \frac{d\bar{v}}{dy} + \frac{d\bar{w}}{dz}\right)$$
$$= \left(e^2 + \mathrm{P}\frac{de}{dx} + \mathrm{Q}\frac{de}{dy} + \mathrm{R}\frac{de}{dz}\right) + \left(m^2 + \alpha\frac{dm}{dx} + \beta\frac{dm}{dy} + \gamma\frac{dm}{dz}\right)$$
$$- \frac{1}{2}\frac{1}{\mathrm{V}^2}\frac{d^2}{dt^2}(\mathrm{P}^2 + \mathrm{Q}^2 + \mathrm{R}^2 + \alpha^2 + \beta^2 + \gamma^2),$$

[* The reason does not appear.]

which is satisfied by $c = 0$, $m = 0$:—or if e and m exist, by

$$e^2 + P\frac{de}{dx} + Q\frac{de}{dy} + R\frac{de}{dz} = 0;$$

and

$$m^2 + \alpha\frac{dm}{dx} + \beta\frac{dm}{dy} + \gamma\frac{dm}{dz} = 0;$$

and

$$\frac{d\bar{u}}{dx} + \frac{d\bar{v}}{dy} + \frac{d\bar{w}}{dz} = 0, \quad \frac{d^2p_1}{dx^2} + \frac{d^2p_1}{dy^2} + \frac{d^2p_1}{dz^2} = 0;$$

and

$$\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} - \frac{1}{V^2}\frac{d^2}{dt^2}\right)(P^2 + Q^2 + R^2 + \alpha^2 + \beta^2 + \gamma^2) = 0,$$

which means that energy is propagated with a velocity = V, and so the assumed relations connecting P, Q, R and α, β, γ mean little more than that the initial state is stable.

This, I think, shows that, so far, the ether may be a turbulent liquid.

If we compare the dimensions of the quantities involved in the theory of motion of a turbulent liquid with those in the electromagnetic theory, we find it convenient to put these latter dimensions into the following forms, as they are the same on the electromagnetic and electrostatic systems.

Calling $[K^{\frac{1}{2}}\mu^{\frac{1}{2}}] = [V^{-1}]$, and density $[\rho] = [ML^{-3}]$, we can write

Electric displacement $= [K^{\frac{1}{2}}\rho^{\frac{1}{2}}V]$,

Electric force $= [K^{-\frac{1}{2}}\rho^{\frac{1}{2}}V]$,

Magnetic displacement $= [\mu^{\frac{1}{2}}\rho^{\frac{1}{2}}V]$,

Magnetic force $= [\mu^{-\frac{1}{2}}\rho^{\frac{1}{2}}V]$.

It is at once evident that the products of the force and displacement are, in each case, of the dimensions of the P^2, Q^2, R^2, α^2, β^2, γ^2, involved in the theory I have already given.

I think it would be well, perhaps, to introduce some new quantities of zero dimensions to define the polarization of the medium. It seems likely that the velocity involved in P must

depend on how intensely the turbulency is polarized, and could therefore be measured by a quantity of zero dimensions multiplied by a measure of the turbulency. This measure would be $\rho^{\frac{1}{2}}V$, so that, for electrostatic energy

$$P^2 = P_0^2(\rho V^2),$$

and for electromagnetic

$$\alpha^2 = \alpha_0^2(\rho V^2),$$

where P_0 and α_0 were of zero dimensions.

In order to introduce the effect of alterations in material, we may put these in the form

$$P^2 = \frac{\rho P_0^2}{K\mu} \qquad \alpha^2 = \frac{\rho \alpha_0^2}{K\mu},$$

and then the electric displacement will be

$$= \sqrt{\frac{\rho}{\mu}} . P_0,$$

and the electric force

$$= \sqrt{\frac{\rho}{\mu}} . \frac{P_0}{K};$$

while the magnetic displacement will be

$$= \sqrt{\frac{\rho}{K}} . \alpha_0,$$

and the magnetic force will be

$$= \sqrt{\frac{\rho}{K}} . \frac{\alpha_0}{\mu}.$$

If we represent the six quantities $\bar{u}^2$, $\bar{v}^2$, $\bar{w}^2$, $\bar{v}\bar{w}$, $\bar{w}\bar{u}$, $\bar{u}\bar{v}$ by a, b, c, f, g, h, they are connected with the six quantities, P, Q, R, α, β, γ, and the three undisturbed values of $\bar{u}^2 = A$, $\bar{v}^2 = B$, $\bar{w}^2 = C$, by the equations

$$a = A + P^2 + \alpha^2, \text{ &c.}, \quad f = QR + \beta\gamma, \text{ &c.}$$

In order to examine how these are related, take an ellipsoid defined by

$$ax^2 + by^2 + cz^2 + 2fyz + 2gzx + 2hxy = d,$$

and we see that, if A = B = C = R, as is the case except in a crystalline medium, the ellipsoid is

$$R(x^2 + y^2 + z^2) + (Px + Qy + Rz)^2 + (ax + \beta y + \gamma z)^2 = d;$$

so that, if $Px + Qy + Rz = L$, $ax + \beta y + \gamma z = \lambda$,

then $L + i\lambda$ and $L - i\lambda$ are the imaginary circular sections of the ellipsoid,* and consequently the intersection of L and λ, whose direction cosines are proportional to $Q\gamma - R\beta$, &c., is the major axis of the ellipsoid, when the above signs are attributed to L^2 and λ^2. As any ellipsoid can be expressed in this form by referring it to its circumscribing sphere and the corresponding planes of circular section, it is apparent that any polarized state of the turbulent motion can be built up of P, Q, R, and α, β, γ polarizations. The axis of the ellipsoid mentioned above represents the flow of energy in the medium during the propagation of a disturbance.

I am inclined to think that Sir William Thomson's fear that diffusion would vitiate these investigations would be avoided either by supposing the turbulent liquid to consist of interlocked vortex rings, or of infinite intercrossing lines; and in either case a natural hypothesis would be that matter consisted of free vortex rings.

* This was remarked to me by Professor Lyle, of Melbourne, while I was recently speaking to him upon this subject.

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MULTIPLE RESONANCE OBTAINED WITH HERTZ'S VIBRATORS

[Conjointly with Dr. F. T. Trouton, F.R.S. From *Nature*, January 30, 1890.]

WHILE Mr. Trouton and I were carrying out some experiments to try and drive an independent current through the arc formed when a spark passes in a Hertzian resonating receiver, we succeeded to some extent in doing so, but obtained an unexpected result which may be of service to others working upon this matter. We found that if the two sides of the receiver be connected with a delicate galvanometer, it is affected whenever a spark passes. It is not so easy to get sparks to pass when the galvanometer is so connected as when the receiver is insulated; but whenever a spark passes, the galvanometer—a 7000-ohm Cambridge Scientific Instrument Company's pattern—is deflected through several degrees and often off the scale. It is not very easy to see how the action takes place, because one would imagine that an electro-dynamometer would be required. The current is reversed along with the reversal of the primary induction, and seems to be connected with the direction of the electromagnetic impulse that first breaks down the air-space in the receiver: an explanation founded upon this consideration explains the facts so far, but further investigation is required to fully confirm it. We have found this method of observing the Hertzian phenomena, which we have worked successfully with an apparatus giving a wave-length of 0·6 metre, much more satisfactory than the method founded on utilizing the conductivity of the spark as a path to drive an independent current either

across or along. Some experiments in a vacuum tube, however, showed that this method is capable of extension. We found it also more satisfactory than a bolometer method, which, however, worked fairly well. For this we interposed, instead of the spark-gap, a very fine wire, which was made into one of the arms of a Wheatstone's bridge. The great desideratum was a *very* fine wire, and we intend trying silvered quartz fibres if we can obtain them, and lead drawn inside glass, &c., our hearts having been broken trying to use that brittle beauty, Wollaston wire.

Any of these methods, in which your observing apparatus, the galvanometer, can be at a distance from the receiver, is more manageable than ones like that described by Mr. Gregory, in which the receiver is itself also the observing apparatus. We exhibited our method of observing the occurrence of spark by connecting the ends of the spark-gap with a delicate galvanometer at the meeting of the Dublin University Experimental Science Association last November.

[By Dr. Trouton.]

As I see from a notice of the proceedings of the Academy of Sciences, Paris, in last week's Nature (p. 287), that MM. Edouard Sarasin and Lucien de la Rive have observed the fact that "multiple resonance" can be obtained by using different sized resonators with a Hertzian "vibrator," I adjoin the following short account of experiments of a somewhat different character made during last autumn, which have led to the same results, and which were brought before the notice of the Dublin University Experimental Association last November. Since then I have learnt what these experimenters also seem not to have known—that some of Hertz's earlier experiments were more especially concerned with this very fact.

First, it was found that the wave-length in the Hertzian experiment of loops and nodes, formed by reflection from a large metallic sheet, had altered since the apparatus had been last

used some months previously. This was attributed at first to something in the "vibrator," such as the width of the spark-gap; but ultimately, on remembering how an accident had necessitated a new resonator being made, the cause was recognized—namely, that it was not exactly the same size as the previous one; and when several resonators of different sizes were made, they were found to give the node at different distances from the reflecting sheet. The intensity of the sparking with which these were affected increased with their size up to a certain point, and then diminished. So that it seems as if a "vibrator" did not send out a "line spectrum," so to speak, but sends out a "band spectrum," the centre of which is the brightest. The period, then, of a "vibrator" is that belonging to the centre of this "band."

Again, in like manner, a "resonator" was always found to give the node in different positions according to the size of the "vibrator" employed. This is what would be expected from the principle of resonance, a resonator being able to respond to any member of the "band" it would itself give out when acting as a radiator, the central period of course with the greatest ease. Some such factor as

$$e^{-(\lambda - \lambda_0)^{m\epsilon}}$$

could, perhaps, express this sort of thing, where λ belongs to the period of the radiation, supposed for the moment "monochromatic," falling on the resonator, and λ_0 belongs to the "period" of the resonator, or that of the centre of its "band."

The position of the node was also found to vary on altering the character of the dielectric surrounding the resonator: even laying a piece of sealing-wax on the wire of the resonator was sufficient to be observed. This might be employed to determine "V" in a dielectric of which only a small quantity was obtainable.

It is obviously of importance for the "central period" of the resonator employed to coincide with that of the vibrator, especially when determining the velocity of the disturbance, for this

is presumably the period given by theory. This is practically always done when arranging their relative sizes, so as to obtain greatest intensity. So that the caution urged by M. Cornu in connexion with Prof. Hertz's measurements of this velocity seems, from these considerations, to be to a great extent unnecessary.

It would obviously be of importance to investigate what form the resonator should take, so as to give out a radiation most approaching one definite period.

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ELECTROMAGNETIC RADIATION

[From the *Proceedings of the Royal Institution of Great Britain.* Discourse delivered on Friday evening, March 21, 1890.]

In order to discover whether actions are propagated in time or instantaneously, we may employ the principle of interference to measure the wave-length of a periodic disturbance, and determine whether it is finite or no. This is the principle employed by Hertz to prove experimentally Maxwell's theory as to the rate of propagation of electromagnetic waves. In order to confine the experiments within reasonable limits we require short waves, of a few metres' length at most. As the highest audible note gives waves of five or six miles long, and our eyes are sensitive only to unmanageably short waves, it is necessary to generate and observe waves whose frequency is intermediate between them, of some hundred million vibrations per second or so. For this purpose we may use a pair of conducting surfaces connected by a shorter or longer wire, in which is interposed a spark-gap of some few millimetres' length. When the conductors are charged by a coil or electrical machine to a sufficiently high difference of potential for a spark to be formed between them, they discharge in a series of oscillations, whose period for systems of similar shape is inversely proportional to the linear dimensions of the system so long as the surrounding medium is unaltered. When the surrounding non-conducting medium changes, the period depends on the electric and magnetic specific inductive capacities of this medium. Two such systems were shown. A large one, whose frequency was about 60 millions

per second; and a small one, whose frequency was about 500 millions per second. The large one consisted of two flat plates, about 30 cm. square and 60 cm. apart, and arranged in the same way as is described by Prof. Hertz in Wiedemann's "Annalen," April, 1888. The smaller vibrating system consisted of two short brass cylinders terminating in gilt brass balls of the same size, and arranged in the same way as the smaller system described by Professor Hertz in Wiedemann's "Annalen," March, 1889. This latter system was placed in the focal line of a cylindrical parabolic mirror of thin zinc plate, such as that described by Prof. Hertz in this paper.

These generators of electromagnetic oscillations may be called electric oscillators, as the electric charge oscillates from end to end. A circle of wire, or a coil in which an alternating current ran, or, if such a thing were attainable, a magnet alternating in polarity, might be called a magnetic oscillator. A ring-magnet with a closed magnetic circuit is essentially an electric oscillator, while a ring of ring-magnets would be essentially a magnetic oscillator again. The elementary theory of a magnetic oscillator can be derived from that of an electric oscillator by simply interchanging electric and magnetic force. Electricity and magnetism would be essentially interchangeable if such a thing existed as magnetic conduction. The only magnetic currents we know are magnetic displacement currents and convection currents, such as are used in unipolar and some other dynamos. It is in this difference that we must look for the difference between electricity and magnetism.

In order to observe the existence of these electromagnetic oscillations we can employ the principle of resonance to generate oscillations in another system whose free period of oscillation is the same. A magnetic receiver may be employed, consisting of a single incomplete circle of wire broken by a very minute spark-gap, across which a spark leaps when the oscillations in the wire become sufficiently intense. In order that a large audience may observe the occurrence of sparks, the terminals of a galvanometer circuit were connected one with one side of the spark-gap and

the other with a fine point which could be approached very close to the other side of the spark-gap. It was observed that when a spark occurred in the gap, a spark could also be arranged to occur into the galvanometer circuit, and with a delicate long-coil galvanometer (that used had 40,000 ohms resistance) a very marked deflection can be produced whenever a spark occurs. This arrangement we have only succeeded in working comparatively close to the generator, because the delicacy required in adjusting the two spark-gaps is so great. It can, however, be employed to show that the sparks produced in this magnetic resonant circuit are due to resonance, by removing this receiver from the generator to such a distance that sparks only just occur, and then substituting for the single circuit a double circuit, which, except for resonance, should have a greater action than the single one, but which stops the sparking altogether. An electric receiver was also used which was identical with the generator, and had a corresponding, only much smaller, spark-gap between the two plates. When the plates are connected with the terminals of the galvanometer, upon the occurrence of each spark the galvanometer is deflected. It is not so easy to obtain sparks when the plates are connected with the galvanometer as when they are insulated, and it is this that has limited the use of this method of observation. By making the first metre or so of the wires to the galvanometer of extremely fine wire, so as to reduce their capacity, we have found that the difficulty of getting sparks is less than with thick wires. We have not observed any effect due to the thickness of the wires after a short distance from the receiver.

In the case of the small oscillator, a receiver exactly like the one described by Prof. Hertz in his second paper already quoted was placed in the focal line of a cylindrical parabolic mirror, and its receiving wires were connected with the wires leading to the galvanometer by some very fine brass wire. With the large sized generator and receiver, which were placed about 3 metres apart, it was shown that the sparking was stopped by placing a thin zinc sheet so as to reflect the radiations from a point

close behind the receiver. By means of a long indiarubber tube hung from the ceiling it was shown how, when waves are propagated to a point whence they are reflected, the direct and reflected waves interfering produce a system of loops and nodes, with a node at the reflecting point. It was explained that these nodes, though places of zero displacement, were places of maximum rotation, and that the axis of rotation was at right angles to the direction of displacement. It was explained that an analogous state of affairs existed in the electromagnetic vibrations. If the electric force be taken as analogous to the displacement of the rope, the magnetic may be taken as analogous to its rotation, and the two are at right angles to one another. In the ether the electric node is a magnetic loop, and *vice versâ*. Though the two are separated in loops and nodes, they exist simultaneously in a simple wave-propagation, just as in a rope when propagating waves in one direction the crest of maximum displacement is also that of maximum rotation. It was explained that by placing the reflector at a quarter of a wave-length from the receiver this would be at an electric loop, and have its sparking increased. It may thus be shown that there are a series of loops and nodes produced by reflection of these electromagnetic forces, like those produced in any other case of reflected wave-propagation. This was Hertz's fundamental experiment, by which he proved that electromagnetic actions are propagated in time, and by some approximate calculations he verified Maxwell's theory that the rate of propagation is the same as that of light. It follows that the luminiferous ether is experimentally shown to be the medium to which electric and magnetic actions are due, and that the electromagnetic waves we have been studying are really only very long light waves.

A rather interesting deduction from Maxwell's theory is that light incident on any body that absorbs or reflects it should press upon it and tend to move it away from the source of light. Illustrating this, an experiment was shown with an alternating current passing through an electromagnet, in front of which a

good conducting plate of silver was suspended. When the alternating current was turned on, the silver was repelled. It was explained, that as the silver could only be affected by what was going on in its own neighbourhood, and that if sufficiently powerful radiations from a distant source were falling on the silver it would be acted on by alternating magnetic forces, this experiment was in effect an experiment on the repulsion of light, which was too small to have been yet observed even in the case of concentrated sunshine. These slow vibrations are not stopped by a sheet of zinc, though much reduced by a magnetic sheet like tin-plate, though the rapid ones are quite stopped by either—thus showing that wave-propagation in a conductor is of the nature of a diffusion.

In all cases of diffusion, when we consider the limits of the problem, terms involving the momentum of the parts of the body must be introduced. It appears from elementary theories of diffusion as if it were propagated instantaneously, but no action can be propagated from molecule to molecule, in air, for instance, faster than the molecules move, *i.e.* at a rate comparable with that of sound. In electromagnetic theory corresponding terms come in by introducing displacement currents in conductors, and it seems impossible but that some such terms should be introduced, as otherwise electromagnetic action would be propagated instantaneously in conductors. The propagation of light through electrolytes, and the too great transparency of gold leaf, point in the same direction.

The constitution of these waves was then considered, and it was explained that if magnetic forces are analogous to the rotation of the elements of a wave, then an ordinary solid cannot be analogous to the ether because the latter may have a constant magnetic force existing in it for any length of time, while an elastic solid cannot have continuous rotation of its elements in one direction existing within it. The most satisfactory model, with properties quite analogous to those of the ether, is one consisting of wheels geared with elastic bands. The wheels can rotate continuously in one direction, and their

rotation is the analogue of magnetic force. The elastic bands are stretched by a difference of rotation of the wheels, and introduce stresses quite analogous to electric forces. By making the elastic bands of lines of governor balls, the whole model may have only kinetic energy, and so represent a fundamental theory. Such a model can represent media differing in electric and magnetic inductive capacity. If the elasticity of the bands be less in one region than another, such a region represents a body of higher electric inductive capacity, and waves would be propagated more slowly in it. A region in which the masses of the wheels was large would be one of high magnetic inductive capacity. A region where the bands slipped would be a conducting region. Such a model, unlike most others proposed, illustrates both electric and magnetic forces and their interrelations, and consequently light-propagation.

In the neighbourhood of an electric generator the general distribution of the electric and magnetic forces is easily seen. The electric lines of force must lie in planes passing through the axis of the generator, while the lines of magnetic force lie in circles round this axis and perpendicular to the lines of electric force. It is thus evident that the wave is, at least originally, polarized. To show this, the small-sized oscillators with parabolic mirrors were used, and a light square frame, on which wires parallel to one direction were strung, was interposed between the mirrors. It was shown that such a system of wires was opaque to the radiation when the wires were parallel to the electric force, but was quite transparent when the frame was turned so that the wires were parallel to the magnetic force. It behaved just like a tourmaline to polarized light. It is of great interest to verify experimentally Maxwell's theory that the plane of polarization of light is the plane of the magnetic force. This has been done by Mr. Trouton, who has shown that these radiations are not reflected at the polarizing angle by the surface of a nonconductor, when the plane of the magnetic force in the incident vibration is perpendicular to the plane of incidence, but the radiations are reflected at all angles of incidence

when the plane of the magnetic force coincides with the plane of incidence. Thus the long-standing dispute as to the direction of vibration of light in a polarized ray has been at last experimentally determined. The electric and magnetic forces are not simultaneous near the oscillator. The electric force is greatest when the electrification is greatest, and the magnetic force when the current is greatest, which occurs when the electrification is zero: thus the two, when near the oscillator, differ in phase by a quarter of a period. In the waves, as existing far from the oscillator, they are always in the same phase. It is interesting to see how one gains on the other. It may be worth observing again that though what follows deals with electric oscillators, the theory of magnetic oscillators is just the same, only that the distribution of magnetic and electric forces must be interchanged. Diagrams drawn from Hertz's figures published in Wiedemann's "Annalen" for January, 1889, and in "Nature" for March 7th, 1889, and in the "Philosophical Magazine" for March, 1890, were thrown on the screen in succession; and it was pointed out how the electric wave, which might be likened to a diverging whirl ring, was generated, not at the oscillator, but at a point about a quarter of a wave-length on each side of the oscillator, while it was explained that the magnetic force wave starts from the oscillator. It thus appears how one gains the quarter period on the other. The outflow of the waves was exhibited by causing the images to succeed one another rapidly by means of a zoetrope, in which all the light is used and the succession of images is formed by having a separate lens for each picture and rotating the beam of light so as to illuminate the pictures in rapid succession.

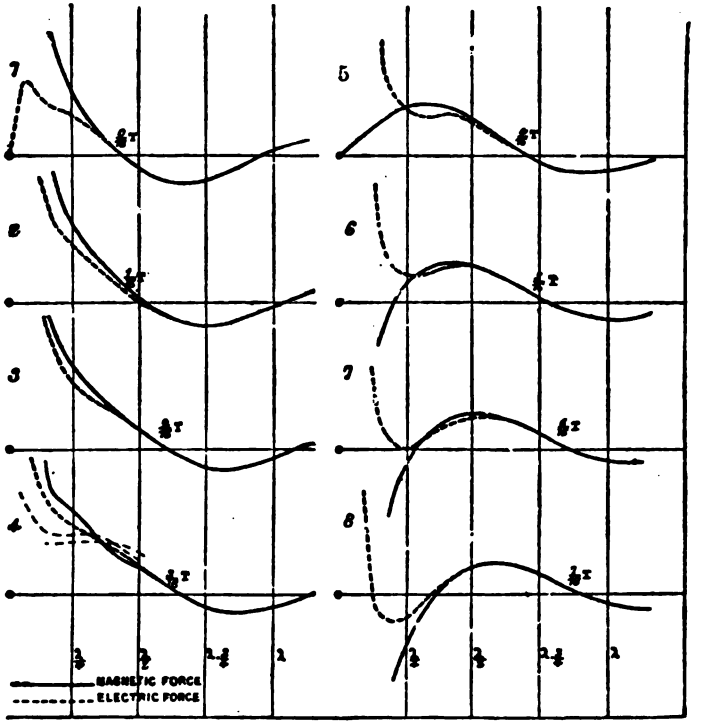
As the direction of flow of energy in an electromagnetic field depends on the directions of electric and magnetic force, being reversed when either of these is reversed, it follows that in the neighbourhood of the oscillator the energy of the field alternates between the electric and magnetic forms, and that it is only the energy beyond about a quarter of the wave-length from the oscillator which is wholly radiated away during each

vibration. It follows that in ordinary electromagnetic alternating currents at from 100 to 200 alternations per second, it is only the energy which is some 3000 miles away which is lost. If an electromagnetic wave, having magnetic force comparable to that near an ordinary electromagnet, were producible, the power of the radiation would be stupendous. If we consider the possible radiating power of an atom by calculating it upon the hypothesis that the atomic charge oscillates across the diameter of the atom, we find that it may be millions of millions of times as great as Prof. Wiedemann has found to be the radiating power of a sodium atom in a Bunsen burner, so that if there is reason to think that any greater oscillation might disintegrate the atom, it is evident that we are still a long way from doing so. It is to be observed that ordinary light-waves are very much longer than the period of the vibration above referred to. Dr. Lodge has pointed out that quite large oscillators in comparison to molecules, namely, about the size of the rods and cones in the retina, are of the size to resound to light-waves of the length we see, and so might be used to generate such waves. This seems to show that the electromagnetic structure of an atom must be more complicated than a small sphere or other simple shape with an oscillating charge on it; for the period of vibration of a small system can be made long by making the system complex, *c. g.* a small Leyden jar of large capacity with a long wire wound many times round connecting its coats, could easily be constructed to produce electromagnetic waves whose length would bear the same proportion to the size of the jar as ordinary light-waves do to an atom. The rate at which the energy of a Hertzian vibrator is transferred to the ether is so great that we would expect an atom to possess the great radiating power it has. This shows, on the other hand, how completely the vibrations of an atom must be forced by the vibrations of the ether in its neighbourhood; so that atoms, being close compared with a wave-length, are, in any given small space, probably in similar phases of vibration. It is interesting to consider this in connexion

with the action of molecules in collision, as to how far the force between molecules after collision is the same as before. In the same connexion the existence of intra-atomic electromagnetic oscillations is interesting in the theories of anomalous dispersion. An electromagnetic model of a prism with anomalous dispersion might be constructed out of pitch, through which conductors, each with the same rate of electromagnetic oscillation, were dispersed. In theories of dispersion a dissipation of energy is assumed, and it may be the radiation of the induced electromagnetic vibrations. These can evidently never be greater than the incident electromagnetic vibration, on account of this radiation of their own energy. In some theories a vibration of something much less than the whole molecule is assumed, and the possibility of intra-atomic electromagnetic oscillations would account for this. Some such assumption seems also required, in order to explain such secondary, if not tertiary, actions as the Hall effect and the rotation of the plane of polarization of light, which are, apparently at least, secondary actions due to a reaction of the matter set in motion by the radiation on this radiation.

Some further diagrams were exhibited, plotted from Hertz's theory by Mr. Trouton, to whom much of the matter in this paper is due. They are here reproduced, and show eight simultaneous positions of the electric and magnetic waves during a semi-oscillation of an electric oscillator. The dotted line shows the electric force at various points, and the continuous line the magnetic force. In the first diagram the magnetic force is at its maximum near the origin, while the electric force there is zero. In the second the magnetic energy near the origin has partly turned into electric energy, and consequently electric force begins. The succeeding figures show how the magnetic force decreases near the origin, while the electric force grows and the waves already thrown off spread away. The change of magnetic force between figures 4 and 5 is so rapid, that a few dashed lines, showing interpolated positions, are introduced to show how it proceeds. It will be observed how a

hollow comes in the line showing electric force, which gradually increases, and, crossing the line of zero force at about a quarter of a wave-length from the origin, is the source of the electric wave, which, starting with this odds, picks up and remains thenceforward coincident with the magnetic wave. From this origin of electric waves they spread out along with the magnetic waves, and in towards the origin, to be reproduced again from this point on the next vibration. These electric and magnetic



forces here shown as coincident are, of course, in space in directions at right angles to one another as already explained. The corresponding diagrams for a magnetic oscillator are got by interchanging the electric and magnetic forces.

A further experiment was shown to illustrate how waves of transverse vibration can be propagated along a straight

hollow vortex in water. It was stated that what seemed a possible theory of ether and matter was that space was full of such infinite vortices in every direction, and that among them closed vortex-rings represented matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains are but different motions. Where alone we can know what motion in itself is, that is, in our own brains, we *know* nothing but thought. Can we resist the conclusion that all motion is thought? Not that contradiction in terms, unconscious thought, but living thought; that all Nature is the language of One in whom we live, and move, and have our being.

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ON THE DRIVING OF ELECTROMAGNETIC VIBRATIONS BY ELECTROMAGNETIC AND ELECTROSTATIC ENGINES.

[From *Nature*, February 11, 1892. Read before the Physical Society, January 22, 1892.]

THE author pointed out that as the electromagnetic vibrations set up by Leyden jar or condenser discharges die out very rapidly, it was very desirable to obtain some means whereby the vibrations could be maintained continuously. Comparing such vibrations with those of sound, he said the jar discharges were analogous to the transient sound produced by suddenly taking a cork out of a bottle; what was now required was to obtain a continuous electromagnetic vibration analogous to the sound produced by blowing across the top of a bottle-neck. In other words, some form of electric whistle or organ-pipe was required. These considerations led him to try whether electromagnetic vibrations could be maintained by using a discharging circuit part of which was divided into two branches, and placing between these branches a secondary circuit tuned to respond to the primary discharge. This did not prove successful, on account of there being nothing analogous to the eddies produced near an organ-pipe slit. The analogy could, he thought, be made more complete by utilizing the magnetic force of the secondary to direct the primary current first into one of the two branches and then into the other. If spark-gaps be put between two adjacent ends of the branches and the main wire, then the magnetic effect of the secondary current should cause the spark to take the two possible paths alternately. Electrically-driven

tuning-forks and vibrating spirals were cases in which magnetic forces set up vibrations, but here the frequency depended on the properties of matter, and not on electrical resonance. The frequency of delicate reeds could, however, be controlled by resonance cavities with which they were connected, and he saw no reason why the same action could not be imitated electromagnetically, using an electric spark as the reed. Referring to the properties of iron in connexion with electromagnetic vibrations, he pointed out that a prism of steel 1 millimetre long had a period of longitudinal vibration of about one-millionth of a second, and as this was comparable with the rates of electromagnetic vibrations, the immense damping effect which iron had on such vibrations might be due to the setting up of sound vibrations in the material. Other methods of driving electromagnetic vibrations had suggested themselves in the shape of series dynamos or alternators. The polarity of a series dynamo driving a magnetic motor would, under certain circumstances, reverse periodically, and thus set up an oscillatory current in the circuit. Similar effects can be got from series dynamos charging cells or condensers. In an experiment made two weeks before, with Planté cells and a Gramme dynamo, reversals occurred every fifteen seconds. Greater frequencies might be expected with condensers. The latter case he had worked out theoretically. He had also tried experiments with Leyden jars and a dynamo, but got no result. This might have been expected, for the calculated frequency was such as would prevent the currents and the magnetism penetrating more than skin deep. Calling the quantity of electricity on the condenser Q, the differential equation for a dynamo of inductance L, and resistance r, and a condenser of capacity x is*

$$L\ddot{Q} + r\dot{Q} + \frac{Q}{x} = -\dot{L}\dot{Q},$$

or

$$L\ddot{Q} + (r + \dot{L})\dot{Q} + \frac{Q}{x} = 0.$$

[* The sign of $\dot{L}$ has been changed.]

If $\dot{L}$ be = 0, the solution of the equation is

$$Q = Q_0 \epsilon^{-\frac{r}{2L}t} \cos 2\pi \frac{t}{T},$$

and the rate of degradation of amplitude depends on the factor

$$\epsilon^{-\frac{r}{2L}t}.$$

If, however, $-\dot{L}$ be greater than r, the exponent of ϵ becomes +, and hence Q would go on increasing until limited by the saturation of the iron or the increased resistance of the conductors due to heating. A dynamo without iron, provided one could be made to run fast enough to send a current through itself, would be likely to give the desired effect. The author thought that by making such a dynamo large enough and its armature very long, it would be possible to get a frequency of about one million. Electrostatic machines seem, however, to be more promising driving agents. Like series dynamos, their polarity depends on the initial charge and can be easily reversed. Hitherto such machines have been inefficient mainly on account of the sparking in them, but Maxwell had shown how this could be obviated. There was the same kind of difference between electromagnetic and electrostatic machines as between Hero's engine and the modern pressure engine. Like modern engines electrostatic machines worked by varying capacity, but the effect of this variation in electrostatic machines was only to vary the frequency and not the rate of degradation. From the fact that electrostatic multipliers could be driven by alternating currents, he thought they might be made to drive alternating currents. If magnetic currents could be obtained, then electrostatic engines would easily be produced. In conclusion the author described a modified electrostatic multiplier which he believed offered a feasible solution of the problem. In this machine the collectors were supposed joined to the ends of the vibrating circuit, and would therefore become + and – alternately. Inductors and brushes were to be so arranged that an insulating cylinder turning between them should have many + and –

charges distributed alternately round its periphery. By suitable adjustment these charges could be collected at the proper instants so as to keep up the vibration.

The chairman, Prof. Lodge, said the paper was very suggestive and full of interesting points. The subject of electromagnetic vibrations was attracting great attention in America in connexion with the manufacture of light. Hertz's oscillations die out too soon to be satisfactory, for their duration rarely exceeds a thousandth part of the interval between consecutive discharges. The theory of dynamos charging condensers he considered extremely interesting, and thought the fact that the damping factor could be changed in sign must have tremendous consequences.

Dr. W. E. Sumpner asked a question about a method of doubling frequency of alternation recently described by Mr. Trouton, in which the armature of one alternator excites the fields of a similar machine. Mr. Trouton had said that after once doubling the frequency it was not possible to go on doing so. He (Dr. Sumpner) thought that by adding other machines the frequency could be still further increased, and gave a proof of the fact. In reply, Professor Fitz Gerald said that adding another machine increased the frequency by a given amount and did not double the preceding one. Hence to increase the frequency a thousand-fold, a thousand machines would be required, and on this account Mr. Trouton considered it impracticable. Prof. S. P. Thompson thought the paper very suggestive, and the acoustic analogies very interesting. Melde's apparatus was an instance of doubling or halving a frequency. On reading the title of the paper, he had expected hearing of a method of maintaining electromagnetic vibrations by giving occasional impulses in some such way as that in which a tuning-fork could be kept vibrating by allowing the hammer of a trembling bell to knock against it. There was another method of intensifying electric oscillations which he had only seen mentioned in a patent specification by Sir W. Siemens, who suggested using a series dynamo with a telegraph cable to augment the signalling currents. On the subject of

ironless dynamos he (Prof. Thompson) desired further information. Some years ago he had made calculations and found the speed at which they would require to run was so enormous as to be beyond the range of engineering possibility. Mr. C. V. Boys, referring to the author's suggestion of using an electric spark with alternate paths to maintain vibration, said that he had tried whether an oscillatory spark was displaced by a magnetic field, but the displacement, even when photographed by a revolving mirror, was barely appreciable. Prof. Perry asked for an explanation of the term "skin-deep magnetism." He was not previously aware that Sir W. Siemens had described a method of improving cable-signalling by using a series dynamo. He himself had patented a somewhat similar arrangement. He had also made a dynamo without iron, but had not got it to work. In reply to Prof. Perry the author of the paper said that in electromagnetic vibrations the magnetic force alternates so rapidly that it could not penetrate far into the field magnet of a dynamo before it is reversed; hence the magnetism would only be skin-deep. Dr. Burton suggested that a commutator with many segments, something like that used by Mr. Gordon in his researches on specific inductive capacity, might possibly be employed for producing high frequencies.

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THE VALUE OF USELESS STUDIES

[From *Nature*, February 25, 1892.]

IT is rather surprising that Prof. Ayrton should indulge in covert sneers at Universities for devoting themselves to useless studies. It certainly ill becomes one whose life is bound up with electrical science, which is of such recent growth that nobody can pretend to forget how it owes its origin to those who studied it while useless. If Universities do not study useless subjects, who will? Once a subject becomes useful, it may very well be left to schools and technical colleges, to patent-mongers, and the trade. Mr. Bury is, on the other hand, mistaken in two respects. That a subject is useless is hardly worth considering as a recommendation for its being made *compulsory* on students. There are too many useless subjects for that. The great objection to compulsory Greek is that it is the principal stumbling-stone in the way of any literature being studied by ordinary University students. The Bible produced very little effect until it was read in translations; and the danger of a pagan revival, if ancient literature were studied without the obstruction of difficult languages, is the best reason for insisting on these languages in a Christian University. The second mistake of Mr. Bury is that it is any part of the business of a University to *teach*. Universities should certainly give facilities for students to *learn*. It is the business of the students to learn. If they are so ill prepared that they have not acquired the art of learning, they should go to a college or school or private teachers, and get taught; for teaching is the business

of these institutions and persons. The business of Universities is to advance culture and knowledge, and to afford students an opportunity of learning how to do this. Prof. Ayrton, by omission, rather than by commission, seems utterly unable to appreciate the value of literature for its own sake. How can all this fierce toil he extols so justly advance a lot of savages?

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M. POINCARÉ AND MAXWELL

[From *Nature*, April 7, 1892.]

In his recent treatise "Électricité et Optique," M. Poincaré professes to give a description of Maxwell's theories of electromagnetic actions. M. Poincaré appears to think that Mossotti's theory is consistent with and differs but little from Maxwell's. On this Maxwell says (§ 62):—"The theory of direct action at a distance is mathematically identical with that of action by means of a medium . . . provided suitable hypotheses be introduced when any difficulty occurs. Thus Mossotti has deduced the mathematical theory of dielectrics from the ordinary theory of attraction." Maxwell anyway repudiated Mossotti's theory. M. Poincaré introduces a "fluide inducteur" as the name of a thing displaced in the dielectric, when what Maxwell calls electric displacement occurs. This is all very well. It is anyway not inconsistent with Maxwell, even though Maxwell says distinctly that he does not know what the change of structure is like which he calls electric displacement. It might be a bending or twisting or lots of things, but M. Poincaré is partially justified in fixing the idea thus. He calls this "fluide inducteur" elastic, though at the same time he calls it incompressible. It is not quite clear what "fluide" means here. M. Poincaré certainly observes that the elasticity of the "fluide inducteur" is quite different from that of material bodies, and in fact acknowledges that it is such as can hardly be fairly attributed to an incompressible fluid. Indeed, how can an incompressible fluid be elastic at all? There must be *something*

besides the fluid; there must be some structure fixed in space which offers an elastic reaction to the fluid when driven past it, or else there must be the two liquids he objects to that are driven past one another. It is hardly a fair representation to talk of *an elastic incompressible fluid*, and then to invent difficulties, when the phenomena could not confessedly be represented by any such thing, but only by a fluid with some other mechanism superadded.

M. Poincaré's statement, "La méthode précedente n'est pas la seule que l'on puisse employer pour déduire de la théorie de Maxwell les lois de la distribution électrique," coupled with his further statement of "une autre méthode ... sans supposer l'existence de ce fluide," seems at variance with his implication that this elastic incompressible fluid is part of or involved in Maxwell's theory.

This leads to the question of how far Mossotti's theory can fairly be considered as a substitution for or as a development of Maxwell's. It does not in any real sense get over action at a distance. There are the horrid old electrical charges acting upon one another across a space full of some non-conducting medium. This is practically no advance as far as a theory of electrical action is concerned. It is an advance no doubt as far as the behaviour of the medium is concerned, inasmuch as it enables a time-propagation through space to occur; but as a theory of electric action it is a distinctly retrograde step on Maxwell's scientific position that he did not know what was the structure of the ether.

M. Poincaré proceeds to criticise Faraday's theory of the stresses in the dielectric, which he attributes to Maxwell. He begins by suggesting that the forces should have been explicable by the elasticity of the inductive fluid, in the same way as mechanical forces are due to the elasticity of matter. He has in this quite forgotten that what he calls the elasticity of this fluid, is not a bit like the elasticity of any matter, and would require either a second fluid, which he rejects, or some structure other than the fluid, to explain its properties.

Granting such an additional structure, then the elastic energy of the medium, fluid and structure combined, *does* exactly explain the motions of conductors. Nobody has explained exactly *how* conductors differ from non-conducting space in structure, and can or do move, and this is not a bit clearer on Mossotti's hypothesis than on any other, not even when the non-conducting diaphragms are made infinitely thin. Maxwell long ago pointed out that no linear system of stress could leave a medium in equilibrium and move bodies immersed in it; and yet M. Poincaré criticises Faraday's system because it is not linear; and this after remarking himself that the elasticity postulated already was not a bit like that of matter. All that is necessary is some assumption as to the connexion between the conducting matter and the dielectric, for the "fluide inducteur" by hypothesis has elastic properties that make it the seat of the right amount of potential energy; and all that can possibly be necessary is to connect the matter with it in such a way that the energy of the medium lost when the conductor moves is given up to the conductor. M. Poincaré has again omitted to remember that the peculiar elasticity of the "fluide inducteur" necessitates some structure with which it is connected, and the Faraday stress may be in this structure, and due to its connexion with the "fluide inducteur," and not at all due to another fluid with peculiar properties. If the stresses are due to the connexions of the "fluide inducteur" there is no great difficulty in supposing them proportional to the squares of the displacements of the "fluide inducteur," just as the increased tension of a stretched horizontal string due to a small weight at its centre is proportional to the square of this weight. In fact, a suggested model working upon this sort of principle has been published as illustrating this very point, and Dr. Lodge's model ethers, in the first part of his "Modern Views," are all of this kind.

M. Poincaré proceeds to find "une difficulté plus grave." He creates this by assuming that the energy of the medium is all due to the work done by these mechanical stresses deforming it. This is a most gratuitous assumption. Take the case of

the stretched string with the weight on it. The increased energy of the system is not due only to the work done by the *increased* tension. At last he confesses, however, that if the energy in the dielectric be kinetic and not potential these difficulties would disappear. "Mais on ne peut encore adopter cette interprétation de la pensée de Maxwell sans se heurter à de grandes difficultés." And why? Merely because Maxwell afterwards calls the electric energy potential while he calls the magnetic energy kinetic. Has M. Poincaré forgotten that potential energy may in any case be the kinetic energy of an associated system? or can he not imagine two modes of motion of the same medium? Anyway, if the potential energy may be the kinetic energy of an associated system, and if M. Poincaré's difficulties are inapplicable to a kinetic explanation of the phenomena, it seems impossible but that they are really inapplicable to a potential system if this system be judiciously devised. It is just here that M. Poincaré fails. He revels in elastic fluids, and yet he continually harps upon the same difficulty—namely, "How can an incompressible liquid be elastic at all?"—and instead of once for all solving this by acknowledging that there must be some structure, he reverts to it as if it were a new difficulty whenever he comes across its consequences.

As a mathematical work the book is admirable and clear, if a little prolix.

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AN ESTIMATE OF THE RATE OF PROPAGATION OF MAGNETIZATION IN IRON.

[From *Nature*, August 10, 1892. Abstract of *Brit. Assoc.* paper.]

ASSUMING that the iron is constituted of a system of little magnets, and with possible assumptions as to the size of these magnets and their strength, it is found that their natural rate of vibration may be one hundred millions per second. Unless the period of the vibration propagated through the iron approximates to this, the wave-lengths would be very small; while quicker vibrations, with periods like those of light, would not be propagated at all.

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UNIVERSITIES AND RESEARCH

[From *Nature*, December 1, 1892.]

AT the discussion in Edinburgh on the proposed National Laboratory, Lord Kelvin and Sir George Stokes took marked exception to my contention that the primary business of Universities was research, contending that it was teaching. In a sense their contention is true, but not in contradistinction to my contention. The distinction would hardly be worth fighting over were it not that they took up the further ground that only those researches should be engaged in in Universities which were likely to interest the students. Of course the leaders of science can if they choose sell the great birthright of Universities for a mess of fees, but I hope they will not be permitted to do so without protest. What view the democracy take of Universities is of the very last importance with our democratic institutions; and I trust all those who have the welfare of the nation at heart will protest against the Universities being turned into coach-houses. In this connexion it is most important to bear in mind the distinction between the functions of Universities and those of schools and colleges. The function of these latter is primarily to teach those who resort to them. The function of the University is primarily to teach mankind. In former days, when the means for distributing information were very imperfect, students used to flock from all sides to learn directly from a great mind. Nowadays the great mind distributes his teaching broadcast. In old days the only way to learn what was being done to advance knowledge was to go

to the place where knowledge was being advanced. Nowadays we read the Transactions of our learned societies at home. But at all times the greatest men have always held that their primary duty was the discovery of new knowledge, the creation of new ideas for all mankind, and not the instruction of the few who found it convenient to reside in their immediate neighbourhood. Not that I desire to minimize the immense importance of personal influence, it is overwhelming; but it is a question quite beside the one at issue, which is whether the advance of knowledge by research, and the teaching of the whole nation by the discoveries made, is not the primary object of Universities rather than the instruction of the few students who gather in their halls: that is the real question at issue between Lord Kelvin, Sir George Stokes, and myself. Are the Universities to devote the energies of the most advanced intellects of the age to the instruction of the whole nation, or to the instruction of the few whose parents can afford them an—in some places fancy—education that can in the nature of things be only attainable by the rich?

In view of the discussion upon the proposed Teaching University for London it is to be hoped that these things will not be overlooked amid the local questions and rival institutions. It is to be hoped on the one hand that those who will have the privilege of learning in the greatest city in the world will not be deprived of the personal influence of its greatest men, by relegating these to some haven of laboratories where no bracing breath of students shall interfere with the inmates. On the other hand it is to be hoped that London will so far honour itself as not to be content until it sees its University a centre of thought and investigation, from which shall radiate new ideas and discoveries to enlighten and benefit the whole nation. Before I close there is a matter of great importance to which I fear sufficient importance is not attached by those who are directing this matter, and that is the great objections there are to mixing up Universities and Colleges with examining boards. We here in Trinity College, Dublin, suffer very much from the

fact that a considerable number of our students never reside here, but only come over for periodical examinations. We only suffer in one way; while if London adopted this abominable arrangement it would suffer in two ways. We suffer because our degree is much less valued than it would be if all our students were compelled to reside. All our students have not that education, got by friction with their fellows and by contact with trained intellects, which no examination can test, and which is such a valuable training; and in consequence our degrees are the less valuable. London would suffer in this way, and it is a very serious way too. In addition to this, London would suffer from the inordinate importance that would be attached to extern examiners if the University examined London and extern students. So far we have escaped this danger; but it is inevitable in London because the extern element there would be large, influential, and organized, while with us it is of little strength. The result would be to perpetuate and intensify that horrible teaching for examinations which is so necessary an evil in the case of the majority of students, but from which the leaders of thought should be exempt. It matters not that the syllabus, nor even that the very questions, are approved by the professor, if the examination is conducted to any serious extent by an independent mind. The student will seek a coach, who will probably teach him very well indeed, but whose whole view of learning will be of the passing-an-examination type, and who will infect his pupil with this miserable disease. Gradually the professor himself will be involved in the vortex, and the whole University will come to look upon the passing of examinations as the end of life for students; and this is the acme of coaching and the bathos of education.

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HEAVISIDE'S ELECTRICAL PAPERS*

[From *The Electrician* of August 11, 1893.]

THESE volumes consist principally of reprints of Papers published in the *Electrician*, and in the *Philosophical Magazine*, and other papers at various dates since 1872 and up to 1892. In the Preface Mr. Heaviside ventures on a most unnecessary apology for publishing them. They are much too valuable scientifically, practically, and historically to be left buried discontinuously in a variety of serials. The editors of the *Electrician* are to be most particularly commended for their public spirit in publishing such long series of Papers which can only have been interesting to a small minority of their readers, but which were so principally because the Papers were pioneers in the subject they treated of.

The Papers are valuable scientifically because they contain a consistent and sound method of attacking electromagnetic problems in consonance with, and in extension of, Maxwell's theory, and because they contain very valuable scientific results deduced by this method from this theory. The Papers are valuable practically because they teach a sound theory of telegraphs and telephones, and of other matters, such as induction balances, which has already resulted in Prof. S. P. Thompson's patent for increasing induction in telegraph wires, and which, there is every prospect, may lead to vast improvements in telegraphy and telephony, and may even make it possible to work a telephone across the Atlantic. The Papers are valuable historically as being a most interesting record of the development,

* *Electrical Papers.* By Oliver Heaviside, F.R.S. 2 vols.

of methods, and scientific views of nature, in an extraordinarily acute and brilliant mind.

Many complain that Browning is unintelligible. Some say that he writes nonsense. His admirers worship him. Oliver Heaviside has the faults of extreme condensation of thought, and a peculiar facility for coining technical terms and expressions that are extremely puzzling to a reader of his Papers. So much so that there seems very little hope he will ever attain the clarity of some writers, and write a work that will be easy to read. In his most deliberate attempts at being elementary, he jumps deep double fences and introduces short-cut expressions that are woeful stumbling-blocks to the slow-paced mind of the average man, when reasoning about concepts that he (the average man) is not very familiar with. Combined with this is a delightful freshness of illustration and brusqueness of expression that makes some parts of his Papers very lively reading indeed. For example, in connexion with the nature of matter, he says: "Even if we resolve all matter into one kind, that kind will need explaining. And so on, for ever and ever deeper down into the pit at whose bottom truth lies, without ever reaching it. For the pit is bottomless." These personal characteristics of style increase very much the historical value of this reprint. It is this historical value that compensates very largely for the considerable amount of repetition which makes the work longer than a connected new work on the subjects included in the Papers would be. The repetition is largely due to the way in which the Papers were published in various serials and in several series. In each much preliminary ground had to be gone over, in order to make the succeeding extensions intelligible to those who might not have seen the preceding series. Repetition in a minor degree arises from the development of method that was proceeding in the author's mind, and that required former results to be retrieved in the light of improved methods and advanced results.

It will be convenient to divide the consideration of those

Papers into remarks on the author's methods, on the theories he has developed, on the practical applications of those theories, and on the innumerable questions as to the nature of things which he raises.

The great advance in method that the author has made is due to his consistently following out the principles that Maxwell laid down in accordance with Faraday's ideas as to the nature of electromagnetic actions. Maxwell had clearly pointed out that the state of the ether at each point depended upon the electric and magnetic forces there, and upon the electric and magnetic displacements they produced. Maxwell, like every other pioneer who does not live to explore the country he opened out, had not had time to investigate the most direct means of access to the country, or the most systematic way of exploring it. This has been reserved for Oliver Heaviside to do. Maxwell's treatise is cumbered with the *débris* of his brilliant lines of assault, of his entrenched camps, of his battles. Oliver Heaviside has cleared those away, has opened up a direct route, has made a broad road, and has explored a considerable tract of country. This maze of symbols, electric and magnetic potential, vector potential, electric force, current, displacement, magnetic force, and induction, have been practically reduced to two, electric and magnetic force. Other quantities it may be convenient, for the sake of calculation, to introduce, but they tell us but little of the mechanism of electromagnetism. The duality of electricity and magnetism was an old and familiar fact. The inverse square law applied to both; every problem in one had its counterpart in the other. Oliver Heaviside had extended this to the whole of electromagnetics. By the assumption of the possibility of magnetic conduction he has made all the equations symmetrical. Every mathematician can appreciate the value and beauty of this. Can anyone be sure that the ether does not ultimately yield to magnetic force? Can we be sure that there is absolutely nothing corresponding to that diffusion of strain that exists in almost every solid? Can we assert positively that there is no diffusion of motion,

such as, Lord Kelvin has pointed out, interferes seriously with vortex hypotheses as to the structure of the ether?

In addition to this he has done his best to popularize vector analysis. For what, to those familiar with Hamilton's notation, seem very insufficient reasons, he has sacrificed a great deal of what was valuable in Hamilton's methods. In order to avoid a negative sign and the occasional use of δ, he has given up the vector interpretation of his symbols. He was driven to this apparently by the semi-metaphysical foundations upon which Hamilton based his system, which, however, can be based upon a series of definitions just as easily as Mr. Heaviside's own system. That one who works with sin, cos, tan, cotan, sec, cosec, sinh, cosh, tanh, cotanh, sech, cosech, I_0 (ix), etc., etc., should be troubled by a δ and a $-$ sign seems too incredible for this latter reason to have really influenced him; but there is no difficulty in understanding how one who is so constantly requiring to see clearly how his symbols represent the facts, should have been deterred by a semi-metaphysical basis from resting on another man's foundation, and should have invented a system for himself with a foundation that he clearly understood.

Running through a good deal of his work, and coming to a definite suggestion in the last volume, is the question of units of measurement. That eternal 4π troubled him as it has troubled everybody else. It is nearly true that the only rule for introducing it is to consider carefully whether it should be in or no, and having come to the conclusion that it should not be in, then immediately introduce it. It is to be regretted that we inherit the misfortunes of our ancestors, and that electrical and magnetic phenomena were known before electromagnetic ones, and that thus the 4π came in so unfortunately; but there is too much capital sunk in the present system for it to have any chance of changing. When people tolerate miles and furlongs, and pecks and bushels and barrels and firkins and hogsheads, etc., etc., how can they be expected to get up any enthusiasm over the eviction of 4π?

In the theory of electromagnetic phenomena the author has

made great advances. He has extended the theory of wave-propagation in complicated media, of waves along wires and into them, and of the effect of the motion of the medium. The questions are too scientifically technical to enter into here, but it must not be concluded that because they are only shortly noticed that therefore they are unimportant. In fact the extension he has given to the theory of the propagation of waves outside and inside telegraph wires is the foundation for a large part of the most important practical applications of electromagnetic theory that he has pointed out.

The most important practical applications of his theory are to the use of induction balances, and to the improvement of telegraph and telephone lines. In this latter field he is unique as an investigator, and every one of his suggestions is worthy of the most serious consideration by those who desire to advance this application of electromagnetic energy. In this connexion he has called particular attention to, and worked out in a very complete manner, the penetration of the current into the conducting wire, the effect of the self-induction in neutralizing the evil effects of capacity, the effect of leakages in improving signals, and the conditions that the terminal receiving instruments must fulfil in order that they may not themselves introduce confusion. The effects of all these, except leakages, are now quite familiar to users of alternating currents, thanks in no inconsiderable degree to Mr. Heaviside. Long before tube conductors were used on the Deptford installation this theory was worked out by him, and equations given for calculating how thick the tubes should be, which should have been usable by properly educated practical electrical engineers, or which should at least have shown them that Mr. Heaviside was the person whom they should consult upon such a point. Long before Mr. Ferranti observed a, to him, new rise of voltage in London, Mr. Heaviside had called attention to the same thing in connexion with telegraph signals, and had shown to what it was due, and on what principles it could be avoided by the use of carefully designed receiving instruments. Long before prac-

tical engineers were at all familiar with the way in which capacity and self-induction produce opposite effects upon the difference of phase between volts and ampères, Mr. Heaviside had shown how to use this in designing telegraph cables in which some of the evil effects of capacity could be to a large extent neutralized by increasing the self-induction. Before Prof. S. P. Thompson took out patents for increasing this self-induction Mr. Heaviside had suggested, and had proved the efficiency of, putting iron filings into the insulation of a cable, had suggested drawing a double wire of iron and copper, and had shown that, by winding iron wire like gimp outside the copper wire, a continuous self-induction might be obtained which would improve the efficiency of telegraph and telephone lines. Everyone is familiar with the way in which self-induction alters the impedance more and more the greater the frequency. What Mr. Heaviside has shown is that by a proper combination of self-induction and capacity the effects on waves of small frequency can be made nearly the same as on those of large frequency, and that consequently speech, which consists of a mixture of waves of all sorts of frequencies, can be much better transmitted than by a line in which either capacity or self-induction preponderates. He has further shown that by a proper regulation of leakage it is possible to still further improve articulation. Telephony, in accordance with Mr. Heaviside's predictions, has advanced so much of recent years beyond what was a few years ago regarded as its limits, that it confessedly requires an amended theory. When the properties of a line have to be determined experimentally, after it has been laid down, it stands confessed that the theory of those who are unable to predict its behaviour is incomplete. Mr. Heaviside is, by the practically unanimous opinion of scientific experts, and by his success as a prophet, pointed out as the authority from whom the amended theory comes, and who should be consulted by all those who desire to improve telegraphic signalling. He is a scientific expert of the best kind in those subjects, and it is very much to be regretted that scientific experts are, in these countries, so highly paid to advance causes and so very seldom

paid to advance practice. The telegraph department has, however, shown itself anxious to improve the means of communication by applications of the most recent scientific ideas, and it is inconceivable that any personal friction should really stand in the way of the utilization of what the Royal Society has crowned with a Fellowship.

The questions of scientific and practical interest raised by Mr. Heaviside are innumerable and most suggestive, as might be surmised from the acute and ingenious nature of his intellect. He has traversed the von Helmholtz theory of a double electrical layer at the contact of dissimilar bodies, and Clausius' theory as to the Thomson effect being due to change of structure. This latter is largely a question of words, and of the meaning of the term "change of structure"; and the author's theory as to the former is surrounded by serious difficulties, as, indeed, is every theory as to the connexion of matter and electricity. There are discussions as to the internal energy of a magnet, which are interesting in connexion with the puzzle as to why the self-induction of a circuit is independent of the magnetic field. He raises questions as to the equilibrium of the ether when transmitting radiations, owing to the pressure exerted by alternating currents upon conductors, as to the flow of energy through the ether, as to magnetic storms and earth currents being connected with solar waves, as to the rate of subsidence of Earth currents, as to the behaviour of metals to light, as to the nature of lightning flashes, as to the effect of the motion of matter through the ether, as to the inertia of electric currents, and as to many other matters. As regards solar waves causing Earth currents, Lord Kelvin has pointed out a serious difficulty in the enormous amount of energy that such a radiation would require, from which it would seem that if magnetic storms are due to solar causes, they must be connected either very indirectly with solar magnetic storms, or else the electromagnetic influence is confined to some ray projected from the Sun, and which happens to cross the Earth; though, here again, it is difficult to see how such a ray can be

produced unless it is a projection of electrical ions in some of the outlying streamers of the corona.

In connexion with this matter of electromagnetic waves, it is interesting to observe that any moving body magnetized like the Earth is continually emitting waves and losing some energy, though, in the case of the Earth, an almost infinitesimal quantity. It would be interesting to consider the problem of the motion of two oppositely electrified bodies round one another. The question would be complicated by the finite rate of propagation of the influence between them, and by the loss of their energy by radiation. In view of the Maxwell light-pressure, would moment of momentum be conserved? or would the problem be practically the Earth-Moon problem with tides absorbing energy? How would the aberration of the force due to the velocities of the bodies and of the force effect the problem? In connexion with the question of the inertia of electric currents we owe it principally to Mr. Heaviside that he has put away the temporary function that Maxwell had introduced as the inertia of the current, and has concentrated attention on the magnetic force as the true source of what is analogous to inertia in electromagnetic questions. In view of the duplex nature of electromagnetic phenomena which Mr. Heaviside more than anybody else has taught us to recognize, it is a little startling to find him stating so very confidently that electric current has *no* inertia, while it would appear from this very duplex property that we might *a priori* have attributed *all* the inertia to the electric force and none to the magnetic just as satisfactorily as the reverse. It is evident, however, that we must know more about the structure of the ether, and possibly of its connexion with matter, before we can finally decide what is the really soundest view of the matter, and that no finally satisfactory answer will be given until all energy has been explained as kinetic energy.

Since Oliver Heaviside has written, the whole subject of electromagnetism has been remodelled by his work. No future introduction to the subject will be at all final that does not

attack the problem from at least a somewhat similar standpoint to the one he puts forward. It is to be hoped that some author with a genius for popular exposition will soon write a "New Electromagnetism" with action at a distance strictly tabooed, with electric pressure and the displacement it produces as the source of the electric energy per unit volume in the ether, and with magnetic momentum and its accompanying flow as the source of the magnetic energy per unit volume in the ether,—with these as the leading ideas of the work. Such a work would be electromagnetism up to date, and its possibility would be largely due to Oliver Heaviside.

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ON THE PERIOD OF VIBRATION OF DISTURBANCES OF ELECTRIFICATION OF THE EARTH

[From *Nature*, September 28, 1893. Abstract of *Brit. Assoc.* paper.]

THE period of oscillation of a simple sphere of the size of the Earth, supposed charged with opposite charges of electricity at its ends, would be about $\frac{1}{17}$th of a second; but the hypothesis that the Earth is a conducting body surrounded by a non-conductor is not in accordance with the fact. Probably the upper regions of our atmosphere are fairly good conductors. In a Geissler tube air is a good conductor, and we know that when part of a gas is transmitting an electrical disturbance the rest of the gas in its neighbourhood becomes capable of transmitting such as well. Extending the analogy, we may assume that during a thunderstorm the air becomes capable of transmitting small disturbances. If the Earth is surrounded by a conducting shell its capacity may be regarded as that of two concentric spheres, and is accordingly greater than that of a simple sphere, which would produce a corresponding change in the rate of oscillation. But at the same time the presence of currents in the outer air would alter the self-induction; and calculation shows that the net result is a comparatively slight change in the period of oscillation. If we assume the height of the region of the aurora to be 60 miles or 100 kilometres, we get a period of oscillation of 0·1 second. Assuming it to be 6 miles (or 10 km.) the period becomes 0·3 second.* On the Sun we

[* For the modes and periods of free vibrations, propagated along the dielectric layers of condensers, *cf.* Larmor, *Proc. Lond. Math. Soc.*, Dec. 1894, p. 131; for a spherical condenser the fundamental period is that required for radiation to traverse the circumference divided by $\sqrt{2}$.]

might expect very much greater periods of oscillation; but these oscillations would not give rise to radiations. If alternating currents of the kind referred to really travelled north and south around the Earth, they would give rise to east and west alternating magnetic forces of periods between $\frac{1}{10}$ and $\frac{3}{10}$ of a second. Dr. Lodge has already looked for evidence of such magnetic forces, but on the assumption that the period would be $\frac{1}{17}$ second. The author has calculated what magnetic disturbances would be produced by given charges. A disturbance equal to $\frac{1}{10}$ part of the horizontal force of the Earth would correspond to an electrostatic charge of 80 C.G.S. units per sq. cm. Such a charge would reduce the superficial pressure on the Earth by an amount corresponding to a weight of 40 gm. per sq. cm. This does not sound probable, but we must remember that it would correspond to a most fearful magnetic storm. A charge of 8 C.G.S. units per sq. c.m. would produce a variation of $\frac{1}{100}$ of H, and would not sensibly affect the barometer. The records of existing magnetic observatories are not sufficiently complete to admit of testing the other suggestions made in the paper.

Professor O. Lodge thought that the detection and observation of such magnetic disturbances was work that could only be done in a National Physical Laboratory. If the Sun were a conducting body surrounded by a non-conductor, the period of an electrical oscillation upon it would be $6\frac{1}{2}$ seconds. He had hung up a needle in his laboratory and watched it for hours, but the only disturbances observed were due to trains and traffic. He pointed out that the electric vibrations of a molecule, calculated from its size, were more rapid than those required to produce light. He suggested the addition of a jacket like that which Prof. FitzGerald assumed to exist around the Earth; but would this prevent radiation?

63

SYSTEMATIC NOMENCLATURE

[Conjointly with Dr. F. T. Trouton, F.R.S. From *Nature*, December 14, 1893.]

WITH reference to Prof. Newcomb's suggested nomenclature for radiant energy, which appeared in NATURE, November 30, p. 100, it seems advisable to be rather cautious in adopting new words, or rather terminations to words already more or less in use, for at the present time the student beginning the study of physical science is fairly bewildered with the various forms of words used under the present system, or rather want of system, in nomenclature.

If once for all some system of termination was settled upo (as in chemistry, for the increasing oxidation results, etc.), the coinage of words as fresh needs arose would proceed automatically on rational lines.

This might very well form the object of a special committee of the British Association.

Mr. Oliver Heaviside's system for electromagnetic matters has much to recommend itself for adoption, also, in general physics.

For example, after the plan (1) conduc*tion*, (2) conduc*tance*, (3) conduc*tivity*, we would have, in the case of radiant energy, (1) radia*tion*, (2) radia*tance*, (3) radia*tivity*.

The first is for reference in a general way to the phenomenon in question; the second refers to its amount in appropriate units in any individual case; while the third is suitable for expressing the peculiar action or factor in the phenomenon possessed by different *kinds* of bodies. Thus the radia*tance*

from a hot kettle would be the total quantity of energy lost per second; the radia*tivity* would be the quantity of this per square centimetre.

With a view of examining the feasibility of this system, the following list is subjoined. Many of the words appear at first as if they would prove most awkward in practice; but remembering similar fears (which subsequently proved groundless) in electromagnetic matters, one is afraid to say they are due to more than unfamiliarity.

Phenomena	Amount of	Coefficient of
Absorption	Absorbance	Absorbivity
Attrition	Attritance	Attritivity
Diffusion	Diffusance	Diffusivity
Emission	Emissance	Emissivity
Expansion	Expansance	Expansivity
Extension	Extensance	Extensivity
Friction	Frictance	Frictivity
Gravitation	Gravitance	Gravitivity
Heat	Heatance	Heativity
Inertia	Inertance	Inertivity
Polarization	Polarizance	Polarizivity
Reflection	Reflectance	Reflectivity
Refraction	Refractance	Refractivity
Rotation	Rotatance	Rotativity
Solution	Solutance	Solutivity

Special attention deserves to be called to inertance as a good name for mass, and inertivity for density, to rotatance for moment of momentum, and rotativity for moment of inertia.

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ON THE RADIATING POWER OF ROUGH SURFACES

[From *Nature*, December 14, 1893.]

IN connexion with this subject there are many things to be considered, and one of the most important is the question of radiation and absorption, which requires a completely new nomenclature to get over very serious ambiguities that at present embarrass the subject. It is very necessary to distinguish between what may be called, on Prevost's theory of exchanges, the total radiatance, from the actually observed loss of energy by radiation, which is, according to this theory, the difference between the total radiatance and the total absorbance. This difference per degree of temperature is very commonly called the radiating power, but this same word is used in quite a different sense when it is attempted to prove, from Prevost's theory of exchanges, that the radiating is equal to the absorbing power, by a consideration of thermal equilibrium. In this latter case the term radiating power means obviously the total radiatance of Prevost's theory.

It may also be worth while calling attention to the theory, given at Nottingham by Lord Rayleigh, as to the absorbivity of rough surfaces being equal to unity. The general idea underlying his investigation is that owing to diffraction the waves amongst the deep corrugations in the surface spread abroad within them, and hardly any of their energy escapes out again. At the time I called his attention to the way a similar theory would explain the radiating power of rough

surfaces, as I have taught here for years back. I am mentioning this now to call attention to an experiment of Magnus, mentioned in Jamin ("Cours de Physique," vol. iii, part 3, p. 113, top line, edition 1881; *Pogg. Ann.*, vol. cxxiv, p. 476), where I have an old note concerning this theory, and which I had forgotten, to the effect that the radiation from platinized platinum was much greater than that from smooth platinum, but that the increase was chiefly in the ultra-red rays, for that the difference between the two plates was almost completely annulled by a plate of alum. This is what would be expected from the above theory, because corrugations that are small enough to affect the ultra-red radiations might still be too large to be anything but a smooth surface for the visible radiations. There is evidently a good deal still to be done on radiativity.

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ON THE CHANGE OF SUPERFICIAL TENSION OF SOLID-LIQUID SURFACES WITH TEMPERATURE

[From *Nature*, January 25, 1894.]

In a recent very interesting communication to the Birmingham Phil. Soc. (*Birm. Phil. Soc. Proc.*, vol. ix, part 1, 1893), upon the effect of a solid in concentrating a substance out of a solution into the superficial film, in accordance with Prof. J. J. Thomson's investigation ("Applications of Dynamics to Physics," p. 191), Dr. Gore has quoted an observation of Pouillet's (*Annales de Chimie*, 1822, vol. xx, pp. 141–162), that when inert powders like silica are mixed with liquids that do not act on them heat is evolved. On the other hand, when the superficial area of contact between a liquid and its gas is increased heat is absorbed. This latter is known to be the case because the superficial tension diminishes with rise of temperature. In the case of the solid-liquid surface being produced, it would appear at first sight to follow that the superficial tension should increase with increased temperature. The matter is, however, somewhat more complicated. When a dry solid is mixed with a liquid we are substituting a solid-liquid surface for a solid-air surface, and from the fact that most liquids soak *up* into a mass of dry powder, we may conclude that the superficial potential energy of the solid-liquid is less than that of the solid-air surface, *i. e.* that more work must be done to separate the liquid from the solid than is developed by the air getting at the solid. *If these actions are reversible*, we may apply the laws of thermodynamics, and conclude that as heat is evolved when the system does

work, *i. e.* when the solid-liquid surface is increased, that it must require more work to separate the solid from the liquid at high temperatures than at low ones; and in the case of silica and water, for instance, that is very much what one would expect from the action of water at very high temperatures on silica. If we could assume that the superficial tension of air-solid were zero, it would follow from this that the superficial tension of a solid-liquid surface is negative, *i. e.* that there is a superficial pressure, and that the liquid has more attraction for the solid than for its own particles, and that this difference increases with increased temperature, *i. e.* the superficial pressure increases.

The whole subject deserves careful investigation and quantitative treatment, but the difficulty of measuring the superficial tensions of solid-liquid surfaces seems almost insurmountable, so that it would be very difficult to verify the theory. Perhaps something might be done with finely divided liquids that did not mix, and whose superficial tensions might be measured.

66

ON EQUILIBRIUM OF VAPOUR PRESSURE INSIDE FOAM

[From *Nature*, February 1, 1894.]

IT is known that the vapour pressure near a curved liquid surface is different from that near a flat surface, being less near a concave surface and greater near a convex surface than near a flat one. Now, inside foam-bubbles the surfaces are approximately flat, except where three bubbles join to form an edge, and along these edges the surface inside any bubble is concave with a very small radius of curvature. How does it happen that equilibrium can exist with the small pressure in these corners, and a larger pressure required near the flat surfaces? In the first place it may be that equilibrium cannot exist, and that all foam is essentially unstable; and it would be almost impossible to disprove this by a direct experiment. If, however, foam can be stable, it seems as if the only conclusion possible were that the flat surfaces will evaporate and thin down, the liquid condensing in the corners, until the flat parts are so thin that they are in equilibrium with a smaller vapour pressure than a thick liquid surface would require. In other words that the vapour pressure near a very thin film may be less than it can be near a thick one at the same temperature. It is evident that inside or outside a solid box stability must be possible, so that the second alternative is the only solution.*

We see a phenomenon of this latter kind in the hygroscopic films that cover glass. Being due to an attraction of the glass for water, there results what I am describing, namely, that in an atmosphere incompletely saturated a film of such a thickness can exist that the vapour-pressure near it is such as corresponds to the existing vapour pressure in the surrounding atmosphere.

[* See, however, J. Willard Gibbs, *Trans. Connecticut Acad.*, iii, 1878, p. 478, and Lord Rayleigh, *Proc. Roy. Inst.*, xiii, 1891, or Collected Papers, iii, p. 441, in which such permanent differences, of tension, and therefore of pressure, are referred to slight differences in the amount of dissolved matter in different parts of the film; *cf.* the films on glass referred to *infra*.]

If we knew the connexion between the thickness of a film and the vapour pressure near it, it would be possible to calculate the shape of a bubble near a corner; the pressure at any point being that due to the thickness diminished by an amount proportionate to the curvature. So that if $f(y)$ be the pressure due to a film of thickness y, and r be the radius of curvature of the surface of the liquid near a corner, we get as the equation of the surface

$$f(y) - \frac{T}{r(\delta - \sigma)} = \text{constant},$$

where T is the superficial tension which may be a function of y, and δ the density of the liquid, and σ of the vapour of the liquid, which latter will depend on the vapour pressure inside the bubble. To determine this and the constant we should know how much liquid we have at our disposal ($V = \int y dx$) to lie round the bubble and to supply vapour inside.

It would appear that inside a cubical box falling freely, for instance, a bubble would always be spherical unless the quantity of liquid [occupying the remainder of the space] were sufficiently small to require the sides of the bubble to be flattened against the sides of the box, *i.e.* unless the volume of liquid were less than $1 - \pi/6$ times the volume of the box.

In connexion with my letter in last week's *Nature*, I may suggest that a possible cause of warming of solid powders when mixed with liquids, is that the solids have already got a film of water over their surfaces, which being on the outside in contact with air, is at least on its outer surface in tension, and that this enormous area of air-liquid film disappears when the solids is immersed in liquid, and that the heat is due to the extinction of this great film. It would require very careful measurements of the heat evolved and estimates of the area of the film concerned, to decide whether this would account for the heating observed. In my former letter I assumed that liquids would soak up into *dry* powders, and that these latter warmed liquids when mixed with them. The suggestion I am now making would account for a warming being produced by damp powders, or by spray or cloud falling into a liquid.

67

BOLTZMANN ON MAXWELL*

[From *Nature*, February 22, 1894.]

THIS second part of Dr. Boltzmann's account of Maxwell's electromagnetic theory is written from a somewhat different point of view from the first part. The first part presents the theory from the mathematical point of view of a dynamical system whose generalised coordinates are known. This one presents the theory from the physical point of view of a continuous medium whose intimate structure is indeed not fully understood, but whose changes of structure can be fully represented by certain vectors. Although Maxwell has presented the subject from both points of view, the one which really determines the form of his work, and that appears to have led him in his investigations, is the physical view of this second part. A purely analytical view is hardly ever as suggestive as a physical and geometrical one. This latter one suggests extensions, suggests advances in a way that purely analytical investigations seldom do. Compare, for instance, Ampère's investigation of the action of elements of currents on one another with Faraday's treatment of the same subject. The latter has suggested the whole of the recent advances, the working of the ether, the identity of light and electromagnetic waves. The former was magnificent, brilliant no doubt, but it was cold and dead.

* *Lectures on Maxwell's Theory of Electricity and Light.* Part ii. By Dr. Ludwig Boltzmann. 8vo, pp. 166. (Leipzig: Barth, 1893.)

In the preface to this second part, Dr. Boltzmann explains the absence of diagrams, marginal notes, etc. which appeared in the former part. He says a friend conveyed to him the valuable criticism that "Your book is dear." He has consequently left out the embellishments that Englishmen love, as being too expensive for the poor German student, and has only left the motto, which costs nothing, to wit:—

> "War es ein Gott, der diese Zeichen schrieb
> Die mit geheimnissvoll verborg'nem Trieb
> Die Kräfte der Natur um mich enthüllen
> Und mir das Herz mit stiller Freude füllen."

And even this he has not taken from that classic lore that Englishmen delight in, but from a German poet.

Although he regrets the dark and inconsequent character of much of Maxwell's work, he congratulates scientific men that this has left them the more to do. For himself, he only claims to be an exponent of Maxwell's views, and hopes that he may succeed in helping students to understand them.

Electromagnetic equations lend themselves to a great variety of interpretations by analogy with displacements of a medium. They are a system of vectors related to one another by a very simple method of derivation each from the last, by the process of what Dr. Boltzmann says the English call "curling." Starting with the vector potential, the magnetic force is its curl, and the curl of the magnetic vector is the time rate of variation of the electric vector. Any one of this system of vectors may be likened to the displacement or velocity of an incompressible medium, and hence we have one system in which the vector potential is so likened, one in which the electric displacement is so likened, and one in which the magnetic displacement is so likened. These latter two analogies have been the favourite ones. Maxwell frequently speaks in terms of the system in which the electric vector is likened to a displacement of an incompressible medium, and the likening of the magnetic vector to a flow is quite common. In these systems electric energy

is generally considered as potential, and magnetic energy as kinetic. Dr. Boltzmann, however, likens the vector potential, which he calls the tonus, and which Mr. Oliver Heaviside relegates to the realms of merely convenient suppositions, to a displacement of the medium; and in accordance with this analogy the magnetic energy becomes potential, and the electric vector, which is proportional to the rate of variation of the vector potential, being thus a velocity, requires the assumption that electric energy is kinetic. An obvious difficulty arises here from the necessity of making an electric current an acceleration which cannot of course be constant for ever. In this connexion it may be worth while observing that the possible existence of one closed surface inside another, with static lines of the electric vector between them, makes it necessary to assume either—(1) that the vector potential represents a twist round its line, and not a displacement along it; or (2) that it is a displacement up some lines and down others; or (3) that there are sources and sinks of the ether where there is electrification, because without sources and sinks we cannot have a continuous flow going on out from a closed inner surface to a closed outer one. If the tonic vector be a twist, the magnetic vector will be of the nature of a Δ^2, and it would be this structure which should be elastically resisted, and not a twist, as Dr. Boltzmann's and Mr. Larmor's assumptions give. This would return somewhat to Mr. Glazebrook's proposal of years ago. Of course, a complex change of structure, such as a combination of 1 and 2, or any other change, such as crystallization in a hemihedral crystal, would be a possible solution:—in a hemihedral crystalline form, because its two ends must differ in sign. Gravity is probably due to a change of structure produced by the presence of matter, which is analogous to a non-hemihedral crystallization because it is always attractive; there seems no reason to suppose that any bodies exist possessing negative gravitation, the supposed levity of the old philosophers.

To the vector potential Boltzmann gives Faraday's name of

electrotonic state at the point, or, shortly, the tonus of the element of volume. The rate of change of this tonus is the electric vector E, and the kinetic energy due to it is the electric energy per unit volume

$$T = K/8\pi . E^2.$$

This tonic strain is accompanied in general by a tonic stress depending on the curl of the tonic vector which is the magnetic vector, H = curl E, and a corresponding potential energy

$$V = \tfrac{1}{2}\nu H^2.$$

All this is most interesting in connexion with Mr. Larmor's recent papers. He uses Maxwell's analysis in which the magnetic energy is kinetic, and consequently assumes the magnetic vector to be a flow, which he has pointed out can exist as an irrotational one without reaction in Mac Cullagh's medium. This same observation of course applies to Dr. Boltzmann's analysis, the difference being that vortex rings would be rings of magnetic current instead of electric current, and atoms would act like electric diads instead of elementary magnets. The existence of unclosed lines of electric vector, however, seems to make this simple interpretation of Dr. Boltzmann's analysis impossible, and as we cannot in general substitute whirl for flow in fluid motion, the vortex ring analysis could not be applied if Dr. Boltzmann's electric vector were interpreted as a whirl; and hence Mr. Larmor's investigation seems confined to the interpretation he has given.

Having assumed that the medium is such as to react elastically against curl of the tonus (τ), and his fundamental equations thus being

$$(1)\ E = \dot{\tau}; \qquad (3)\ T = K/8\pi . E^2$$

$$(2)\ H = \operatorname{curl} \tau; \quad (4)\ V = \tfrac{1}{2}\nu H^2$$

he proceeds to deduce the equation corresponding to (2), namely

$$(5)\ K\dot{E} = \operatorname{curl} H$$

by applying Hamilton's principle to T – V in a way which is well known. He now remarks that in accordance with his dynamical principles $K\dot{E}$ is rate of change of momentum. And he adds to curl H any external impressed forces, which he divides into two classes: (1) those due to reversible causes, such as electromotive of contact, chemical action, etc., represented by F; and (2) those due to irreversible causes, such as ohmic resistance, etc., which are proportional to the electric vector E. He thus obtains this equation in the form

$$K\dot{E} = \text{curl } H - 4\pi C(E + F).$$

It would thus seem as if the electric conduction current were a different thing from a changing electric displacement, though both depending on curl H. This arises from the difficulty noticed above, and seems to require careful consideration. By judicious theories as to the function of the matter in stopping the continual acceleration without uncurling the H, the difficulty can be surmounted. In order to get over all the difficulties of discontinuities at the surfaces of bodies, Dr. Boltzmann assumes that the properties of the ether vary rapidly but continuously in passing across a surface, so that he can assume that these equations apply everywhere.

Depending on his dynamical basis, Dr. Boltzmann has obtained the following dimensions for electromagnetic quantities, which of course, differ entirely from both the electric and magnetic systems of units:

$$[E] = [LT^{-1}], \qquad [K] = [ML^{-3}],$$
$$[H] = [ML^{-1}T^{-2}],^{*} \qquad [\mu] = [M^{-1}LT^{-2}].$$

After a short discussion as to the possibility of founding the science upon a purely analytical basis, by assuming equations and showing that they lead to true results, which is the basis of Hertz's method, and a short criticism of this method as applied by Hertz, Dr. Boltzmann proceeds to show how the old

* This is misprinted in the text, but right in the table of formulæ at the end of the book.

equations of action at a distance and von Helmholtz's work are connected with Maxwell's view of the subject. His treatment of superficial effects by means of a rapid variation of structure of the ether at the surface of solids, seems essentially the same as Mr. O. Heaviside has advocated in opposition to von Helmholtz's double electric layers. All this analytical method is, of course, necessary and interesting in what may be called a transition work, one that concerns those who have been brought up under one school of thought and are entering another; a sort of Epistle to the Hebrews, a college between youth and manhood. It concerns the past rather than the future, towards which we should press, forgetting those things that are behind.

Although Dr. Boltzmann has left out all embellishments, he has had pity on his readers. There are necessarily included in a transition work of this kind innumerable formulæ, of which 168 are frequently referred to, and these he has collected into two folding sheets, each of five folds at the end of his work. This is most considerate. Books dealing with many formulæ might well follow suit, although it certainly is a little terrifying to have 168 formulæ presented as the outcome of the book in a way that necessarily attracts the attention of anyone who thinks of reading it. Those who are frightened by this should, however, recollect that the whole subject of electromagnetism depends on only four very simple equations. Dr. Boltzmann would have much simplified his work if he had adopted any vector symbolism.

It is to be hoped that this part of Dr. Boltzmann's work, as well as the former part, will soon be translated, and so made easily accessible to English students. The work of a great master, the product of a great mind, helps all men who can understand it.

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LENARD ON CATHODE RAYS

[From *Nature*, March 29, 1894.]

THE current number of the *Electrician* contains a note by Professor FitzGerald on a recent Paper of Herr P. Lenard's, which appeared in *Wiedemann's Annalen*. Herr Lenard has continued his interesting observations on the cathode rays in gases under ordinary pressures and in extreme vacua. In the experiments with high vacua, exhaustion was carried on till a coil, capable of giving a spark 15 cm. long in air, could not produce any discharge between terminals sealed into the experimental tube. Herr Lenard estimates that, when he had condensed the mercury vapour in a connected globe by lowering its temperature to – 21° C., the pressure of the remaining gas was about $0{\cdot}03 \times 10^{-6}$ of an atmosphere, or about 0·03 dyne per square centimetre. In a tube in which the exhaustion had been carried to this extent, nothing was visible on the path of the rays except where they impinged on the glass at the opposite end of a tube 150 cm. long, and when there were no magnets near, they were propagated in straight lines. From these and many other interesting observations Herr Lenard concludes that the cathode rays are phenomena in the ether, and are independent of the presence of matter. With reference to this point Professor Fitz Gerald says:—"If this be so they are a most remarkable addition to the properties of the ether. Phenomena that may all be classed under light-propagation are the only known phenomena of propagation in free ether. There is a very essential difference between these cathode rays and ordinary

light-propagation; and only for this these rays might be very rapid ultra-violet waves, which are known to be rapidly absorbed by air and other gases, but which may be able to run the gauntlet of hundreds of thousands of molecules without being finally absorbed, and might, in accordance with the known transparency of gold leaf, be able to penetrate any solid, even though a conductor, because for their extremely rapid vibrations the molecular motions upon which ordinary conductivity depends may be much too slow to have any sensible effect. The fact that seems conclusive against this view is the deflection of the rays by a magnet. These rays are deflected in the same way as a conductor carrying a current of electricity away from the cathode. No such action has ever been observed on rays of light. It would be most natural to explain the action by the presence of the matter which is generally required in order to be acted upon by a magnet. There seems very little reason for supposing that a magnet would act upon electric displacement currents in the ether, even if displacement currents of the straight ray kind were possible in the ether without propagating themselves out sideways with the velocity of light. When we recollect that in the vacua described by Herr Lenard there are still 10^{10} molecules per cubic mm., there does not seem sufficient reason for looking to an unknown property of the ether when there is so much matter present to explain the phenomenon."

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LENARD ON CATHODE RAYS

[From *Nature*, June 21, 1894.]

In a communication to the *Electrician*, Professor Fitz Gerald criticises Herr Lenard's last paper, a short abstract of which appeared in *Nature* for May 31st, 1894, p. 114. In a former note on Herr Lenard's previous paper, Professor Fitz Gerald had pointed out that the experiments so far would be consistent with the supposition that the cathode rays were rays of light of very high frequency, except for the fact that they were deflected by magnetic force, a phenomenon of which we have no other evidence, and which makes it practically impossible at present to suppose that cathode rays are of this nature. It was further pointed out that if this deflection were an action on the emitting surface, and not on the ray, it might be again possible to explain these cathode rays by the supposition that they are rays of light. Herr Lenard's more recent experiments seem entirely at variance with any such supposition; while Professor Fitz Gerald considers that there is nothing in them that, in the same conclusive way, proves that they are not streams of electrified molecules or atoms. From Herr Lenard's observation that the deflection of the ray depends on the pressure of the gas in the tube in which the rays are generated originally, and not on the nature and pressure of the gas in the tube in which deflection takes place, it follows that, if the rays are paths of projectiles, they must either pass through the window, or else be projected from it, by some action which behaves like a blow given to it from the other side. A study of the spectrum of the cathode light might settle whether

any of the molecules actually traversed the partition. The fact that there is no increase in the pressure within the tube only shows that as many molecules traverse the partition in one direction as in the other. Blows delivered on one side of a plate would project molecules from the other side, with different velocities depending on the nature of the blow and on the mass of the molecule, so that a hypothesis of this kind would be quite in accord with Herr Lenard's observations. The fact that the magnetic effect is independent of the mass of the molecule struck is explicable by supposing the electrical charge to be the same for all molecules. Even though sufficient reasons were forthcoming for rejecting the theory that these rays are due to projected molecules, Professor Fitz Gerald considers that there are other possible suggestions which are worthy of consideration, such that they are straight Grotthus chains of molecules which bend under magnetic force. This might account for a velocity of propagation of actions along them, comparable with the velocity of light, without requiring the component matter to move with this velocity. In fact, until the residual matter within the tube has been reduced very much beyond what has been attained, and it has been shown that these phenomena increase instead of diminish, and that there is no very slow projection of the material of the plates, such as darkens the glass in glow-lamps, it will be very difficult to prove that any of the phenomena hitherto observed are due to the ether, and not to the matter present. So far the phenomena described are quite like those that would be due to moving electrified matter, and the actions are quite unlike anything we know of the properties of the ether.

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THE KINETIC THEORY OF GASES

[From *Nature*, January 3, 1895.]

I DO not feel as if those who heard me ask some questions at the British Association at Oxford, about the kinetic theory of gases, exactly understood my difficulties. They are those of an onlooker, and so they may be of general interest. As several of them have been fairly satisfactorily answered, it may be worth while stating the present position of such an onlooker as myself.

In the first place, consider the difficulty as to reversibility and as to the number of possible ways in which a system could be started on a reverse path so as to obtain a *given* initial state. This is, I think, completely answered in the way Mr. Larmor gives in his letter on p. 152. As well as I can recollect, Mr. Culverwell and I had been mutually satisfied by this kind of explanation previous to the meeting at Oxford, and it was not then referred to.

The question of reversibility lately started, as I understand it, has reference to the introduction of the postulate of chance in the deduction of the theorem about H. Mr. Burbury, in his recent letter, has indicated a proof of this theorem, in which he explicitly postulates chances, and so far justifies the possibility of proof on these lines. I understand that Mr. Culverwell is so far satisfied, and only asks for more, *i.e.* an extension of this form of proof to other cases than the simple one of colliding spheres.

Secondly, as regards the solar system, etc., I am not yet

quite clear why a finite number of particles moving about for an indefinitely long time does not satisfy the conditions of the problem as usually stated, just as well as a large number of bodies for a short time. As to the necessity for *collisions* among the parts of a system, I cannot see why the Earth, Moon, Jupiter, and Sun are not, to all intents and purposes of the generalized coordinates, in collision at present and always; and I desire to know why any other kind of collision is required for the application of the investigation. I think I now see, through conversations with Mr. Culverwell, where the existing investigations may fail to apply to solar systems. I may explain my position as follows. It was always, I knew, postulated that more than two particles should not be in collision at once, and I therefore asked how this could be an essential part of the investigation when applied to the case of air near the Earth subject to gravitation. I did not see why the *Earth* was not (so far as the generalized-coordinates investigation was concerned) a particle in collision with every particle of the air during every one of their collisions with one another, and consequently violating the postulate requiring only *two* particles to be in collision simultaneously. I now understand that when dealing with gravitation and such like forces, these are supposed to be directed to *fixed* centres, and that in the case of a large particle like the Earth this is very nearly true, but that it could not be even approximately true if we had three fairly equal particles acting upon one another simultaneously. This may also explain why the equal partition of energy does not hold in the solar system, where the bodies do not act upon one another in pairs but are all always subject to one another's action. This, as I understand, is also the reason why the direct-distance law is not an exception to the equal partition of energy theorem. It also may explain how we can have water and steam in equilibrium with one another, notwithstanding the apparent *uniqueness* of the Boltzmann-Maxwell solution. From experience it would seem that when we can extend the investigation to the case of several bodies in simultaneous collision, we shall find that there

are *three* solutions corresponding to the solid, liquid, and gaseous states. At the same time, some of the very general investigations that seem to me, as a physicist, as if they were intended to apply to complex molecules in collision with one another, and with a partition of energy amongst the atoms, appear to violate the postulate of collisions in pairs; for I find it hard to conceive of these molecular systems of atoms as other than systems, the various parts of which are held together by mutual actions, and which must consequently be considered to be in simultaneous collision with one another. This may be where the spectrum crux fails. Perhaps somebody would be so very kind as to point out where exactly in these generalized-coordinate investigations the postulate of collisions in pairs is used, and so save lazy people like me the trouble of hunting it up.

This raises the third point as to how this difficulty about the spectral lines is to be surmounted. I cannot follow either Mr. Bryan's, or what I understood to be Mr. Larmor's view, that any help can be got by supposing spectral lines to be due to electromagnetic vibrations. The example Mr. Bryan gives of smooth solids of revolution is quite beside the point. In this case there is *no interchange of energy* between rotation round the axis of revolution and the other degrees of freedom. This is quite contrary to what we know to be the case in respect of ethereal and molecular energy. We know that radiations cause bodies to become cooler, and therefore there *is* interchange of energy. This could not be otherwise, as is evident from what we know of the mechanical forces—electric, magnetic, and electromagnetic—that interact between matter and ether. It is rather hard for Mr. Bryan to say that the *onus probandi* lies with physicists to explain exactly *how* transference takes place; surely the fact that transference does not take place is sufficient to prove that a complete theory should take in both sets of coordinates—ethereal as well as material—and I should have thought that those formidable arrays of $dp_1\ dp_2\ \ldots\ dp_n$ etc., of coordinates and momenta to the nth, with dots between to signify their indefinite number, should include everything of

this kind that could possibly be required. Here, however, the postulate of collisions in pairs entirely breaks down, and thus shows a way out of this spectral difficulty. A second way was suggested to me—by whom I forget—at Oxford, namely, that the complicated system of lines that we see in the spectrum—of iron, for instance—are so connected as to their amplitudes, periods, and phases, as to represent only a single coordinate. Anybody who has tried to expand a simple function in Fourier series will easily understand how a very simple motion might produce a fearfully complicated spectrum.

It seems, then, to me that the questions which need solving in our study of the dynamical foundations of thermodynamics at present are (1) how to explain spectra, and (2) how to deal with several bodies in simultaneous collision ?

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THE FOUNDATIONS OF DYNAMICS*

[From *Nature*, January 17, 1895.]

THIS posthumous volume of Hertz's works, edited by Professor Lenard, with a preface by von Helmholtz, has a doubly melancholy interest. It is the last work of Hertz, upon which he was engaged until a few days before his death; and it contains a preface which is almost the last work of von Helmholtz. The pupil died shortly before his master; and by the departure of such a pupil and of such a master, science, and with science mankind, have lost many prospects of advances in the near future.

In his preface, von Helmholtz pays a touching tribute to the genius of his favourite pupil, from whom he hoped most, and who had drunk most deeply of his master's thoughts. In 1878 their intimacy began. At that time difficulties connected with various electrical theories of action at a distance were occupying his thoughts, and he offered a prize for the best essay on induction in non-inductively wound coils. Weber's theory would have involved an inertia of the electric current distinct from the magnetic inertia. The question is still interesting in connexion with discharges between two charged conductors, one of which completely surrounds the other, when a dielectric between them is suddenly made conductive. There is then no magnetic force. Is there *no* inertia? Can a medium

* "The Principles of Dynamics developed on new lines—Hertz's Collected Works," vol. iii, p. 310. (Leipzig: Barth, 1894.)

become *suddenly* conducting? Is a conducting medium homogeneous? Is there inertia of ionic charges which represent the non-homogeneity of the medium? These questions still require answering; but in the seventies, in Germany, Maxwell's idea of magnetic force accompanying displacement currents was not generally received, and Helmholtz's question as to the induction in non-inductively wound coils really had reference to these displacements. Hertz won the prize by showing that at most only 1/20th or 1/30th of the extra-current could be due to electric inertia. By subsequent experiments on the possible effect of centrifugal force on the current in rapidly rotating plates, he reduced this estimate to a very much smaller value. Mr. Larmor has suggested that any centrifugal force may be balanced by a tension in the length of the current, much in the same way that the tension of a running rope will balance centrifugal force in the curves round which it may be running. In every way the subject deserves further investigation, for it is intimately connected with the most fundamental questions as to the nature of electricity and its connexion with matter.

The next thing to which Hertz devoted himself was a prize problem proposed, at von Helmholtz's suggestion, by the Berlin Academy. The problem was to investigate Maxwell's postulate that changing electric displacement was an electric current. This was the bud from which Hertz's great work sprang. Of it von Helmholtz says: "It is a pity we do not possess more such histories of the inner psychological development of knowledge. Its author deserves our sincerest thanks for letting us see so deeply into the inmost working of his thoughts, and for recording even his temporary mistakes. By this work Hertz has settled for ever the question as to electromagnetic actions being propagated by a medium, and the only outstanding question of the kind is as to gravitation, which we do not yet know how to logically explain as other than a pure action at a distance." It thus appears that von Helmholtz to the last was unconvinced as to the probability of any hypothesis like

Le Sage's or Osborne Reynolds's. He seems, on the other hand, to have been satisfied with the possibility of chemical actions being explained either by electromagnetic actions or by actions not at a distance. This latter term, of course, requires explanation as to what "at a distance" means. Any actions other than those of absolutely rigid bodies, such, for instance, as the fairly well established forces of attraction of gaseous molecules for one another, and some of which can hardly be explained either by electricity, magnetism, or gravitation, seem to be actions at a distance that require explanation just as much as gravitation.

Following this short history of the work of his pupil, which, coming from such a master, must have a permanent interest to all, von Helmholtz gives a *résumé* of the last work of Hertz. In it there is attempted a continuously elaborated presentation of a complete and self-dependent system of mechanics, in which each particular application of this science is deduced from a single fundamental law which can of course be itself only assumed as a plausible hypothesis. In order to explain how this is required, von Helmholtz gives a short history of the development of the science of mechanics. The first developments arose from the study of the equilibrium and motion of solid bodies in direct contact with one another, such as the simple machines, the lever, the inclined plane, the pulley. The law of virtual velocities gives the most fundamental general solution of all such problems. Galileo subsequently developed the knowledge of inertia and of moving force as an accelerating agent. It was, however, conceived by him as a succession of blows. Newton was the first who arrived at the notion of force acting at a distance, and its more accurate determination by the principle of action and reaction. It is well known how strenuously he and his contemporaries resisted this idea of pure action at a distance. From this, men developed the methods of treating all problems of conservative forces with constant connexions, whose most general solution is given by d'Alembert's principle. All the general principles of dynamics have been

developed from Newton's hypothesis of permanent forces between material points and permanent connexions between them. It was subsequently found that these laws held even when these foundations could not be proved; and it was thence deduced that all the laws of nature agreed with certain general characteristics of Newton's conservative forces of attraction, although it was not found possible to deduce all these generalizations from one common fundamental principle. Hertz has devoted himself to discovering such a fundamental principle for mechanics, from which all the laws of mechanics hitherto known as universally valid can be deduced; and he has carried out this with great acuteness, and by means of a very remarkable presentation of a peculiarly general kinematic conception. In working it out, he returns to the oldest mechanical theories, and supposes all actions to be by means of rigid connexions. Of course he has to assume that there are innumerable imperceptible masses and invisible motions of these, in order to explain the apparent actions upon one another of bodies that are not in immediate contact. Though he has not given examples of how this may be the case, he evidently builds his expectation, of being thus able to explain natural actions, upon the existence of cyclical systems, rollers, etc., with invisible motions. The justification of such an assumption can only be obtained by its success. Von Helmholtz concludes this interesting preface by remarking how English physicists have so often based their work on dynamical and geometrical suppositions, as, for example, Lord Kelvin and his vortex atoms, Maxwell and his cells with rotating contents. These physicists, he says, "have clearly been more satisfactorily helped by such illustrations, than by the mere most general representations of the facts and their laws as given by the system of physical differential equations. I must confess that I have restricted myself to this latter method of investigation, and have felt most confidence therein; and indeed I might not have arrived at any important results by the methods which eminent physicists such as the three mentioned have employed."

Although so far it seems as if there were very little to choose between the old method of supposing that natural actions can be explained by conservative forces between molecules, and such a system of rigid connexions, Hertz in his introduction shows that he is dissatisfied with the hypotheses of these forces as entities, while von Helmholtz, by his silence, seems to hold the view that the old method was good enough for him. Hertz's method has, however, the advantage of turning our attention to something definite to be investigated and invented, namely, the structure of these rigid connexions. It is apparently very closely related to Osborne Reynolds's and "Waterdale's" suggestions as to the structure of the ether, namely, that it consists of perfectly rigid particles in almost complete juxtaposition, which, whether by their smoothness or by their rolling upon one another, waste no energy in internal heat motions.

In his own preface, Hertz says that he has culled many things from many minds, nothing particular in his work is new; what he presents as new is the arrangement and collocation of the whole, and the logical, or rather philosophical, aspect thereby attained.

To these prefaces there follows a long introduction, in which Hertz reviews and criticises the present foundations of dynamics. The great road by which this domain is now entered is one that was laid by Archimedes, Galileo, Newton, and Lagrange. It is founded on our notions of space, time, force, and mass. Force is introduced prior to motion, as the independent cause thereof. Galileo's notion of inertia only involved space, time, and mass. Newton first introduced all four notions. To this d'Alembert's principle gave the analytical method of treating generally connected systems. Beyond it, all is deduction. Here Hertz introduces a discussion as to the so-called forces of inertia. From his discussing the case of a solid subject to centripetal acceleration by means of a string, the question is much more intricate than if he had taken the case of a body falling freely under gravity, where the force is

applied directly by the Earth to each point of the body, and not, as in the case of the string, distributed to each part by stresses in the solid. Hertz seems to consider that there is some outstanding confusion in applying the principle of equality of action and reaction, and appears to hold that by this principle the action on the body requires some reaction *in the body* whose acceleration is the effect of the force. He does not seem fully to appreciate that action and reaction are always on *different* bodies. From his consideration of this, and from a general review of our conception of force, he concludes that there is something mysterious about it, that its nature is a problem in physics, like the nature of electricity. We have a quite distinct conception of velocity: why not of force? He concludes that the mystery is not due to our not having enough ideas to associate with the word, but to our trying to put too much into it. These mysteries, however, do not invalidate in any way the deductions that have been made; they only require us to seek out a new foundation for our dynamics. He goes on to criticise this method of filling nature with forces of which, being ultimately between molecules, we can have no direct experience. A piece of iron on a table is acted on by gravitation, cohesion, repulsion, magnetic, electromagnetic, electric, and chemical forces. Some of these would drag it to pieces if unbalanced to a nicety by others. Is this a sound view of nature? Can we not get some more attractive one?

A second view may be elaborated by making our fundamental quantities, space, time, mass, and energy. There is no book in which this view of nature is fully and consistently worked out, at least none that Hertz was acquainted with. He sketches how it might proceed. Besides the postulate of the conservation of energy we require some definition of potential energy and experimental relations connecting it with space; and in addition we have a choice of relations with kinetic energy, of which Hertz suggests the choice of the integral form of Hamilton's principle known as that of least action. This is, no doubt, a recondite idea to use as a fundamental postulate, but it only

implicitly involves the idea of force, which then comes in merely as a definition. To this method, which certainly has several great advantages, Hertz makes a number of objections. In the first place he objects that it requires the equations of connexion to be integral equations, and we know such actions as pure rolling of one hard body on another cannot be so expressed. We must, in order to specify the subsequent motion, know the rate of rotation round the normal axis through the point of contact, and this cannot be specified except in terms of differentials. To such motions we cannot apply the proposed principle of least action, and yet we can hardly dispute that such rolling is possible in nature. If we treat it as the limit of frictional sliding, we introduce the whole of the difficulties of force, or of the irregular heat actions which have not yet been fully made amenable to accurate dynamical treatment. Again, difficulties arise as to the foundation of this method. There is great difficulty in specifying energy itself. How can it be satisfactorily measured without returning to the first method, and introducing the idea of force? Some have conceived of energy as a sort of substance; but when we try to form concrete conceptions of what is occurring, we get involved in perplexities. The very existence of two forms of energy is a very serious difficulty. Again, it is doubtful whether it can be sound to consider the integral of least action as a *fundamental* principle. It makes the present depend on the future. It sets the problem to nature to make a certain integral the minimum.

A good many of these objections could be got over by making all energy kinetic, which is what Hertz himself practically assumes in his own method.

This third method begins by assuming only three fundamental quantities, time, space, and mass, and puts aside as non-fundamental, force and energy. In order to explain how nature works, we already do postulate invisible underlying structures in nature. We postulate these in the atoms and molecules of matter. Hertz sees in all actions the working of an underlying structure whose masses and motions are

producing the effects on matter that we perceive, and what we call force and energy are due to the actions of these invisible structures, which he implicitly identifies with the ether.

We must, however, assume certain connexions between the three quantities, time, space, and mass. Between time and mass there is no direct connexion. Space and mass, Hertz considers, are connected by the existence of a given mass at each point of space. He cannot mean here to assume a complete plenum, which would make serious difficulties in the way of the working of what he subsequently assumes to be a structure of rigid bodies; he must include a vanishingly small density at some points, though perhaps he may have had in view the filling of the interstices between his rigid bodies with a fluid. Any way, he goes on to say that some connexion is required between all three quantities, and for this purpose he postulates his great fundamental single law of motion, which he considers is an extension to systems of Newton's first law of motion for a single body; it is that a system, which is unconnected with any others, moves with constant swiftness along one of its straightest paths. "Systema omne liberum perseverare in statu quo quiescendi vel movendi uniformiter in directissimam." In order to understand what Hertz here means by the path of a system, and by its being straight or curved, requires further explanation; but from this principle, which is capable of analytical representation, and from the assumption that the connexions of a system are all rigid, he deduces all the fundamental principles, conservation of areas, momentum, energy, least action, etc. In considering the motion of any part of a system, we find that we may conveniently introduce certain actions of the other parts of the system upon it which are measured by forces, which thus come in as mere definitions. He does not seem to investigate anywhere the question as to the danger of his rigid connexions becoming tangled. Analytically a postulate that the points of two different bodies that act on one another are in contact is easily expressed, but it does not follow that when we come to invent

actual rigid connexions to produce the observed effects, they will do so for any length of time without jamming. It is a seductive theory that gravitation or electrical actions may be due to vortex filaments ending on atoms; but the tangling of the filaments is a very serious difficulty that has not been satisfactorily got over. Hertz does not seem to feel this as a serious difficulty, but he does notice an obvious objection that is sure to be raised, namely, that rigidity in itself postulates forces. To this he replies that rigidity in itself is merely a matter of definition and of fact. How is our view of the fact that two points are at a constant distance apart, improved by saying that there is a force between them? As, however, real bodies are only imperfectly rigid, Hertz concedes that it may be that when we learn more about these invisible connexions, they may turn out not to be absolutely rigid. It is a matter for further investigation. This very same view might have been urged, and has been urged already with reference to actions like gravity. The *law* of gravity can be perfectly well described without any reference to the notion of force. We may say, every element of matter moves towards every other element in the universe with an acceleration inversely proportional to the square of their distances apart. We can describe the law kinetically, just as Hertz proposes to describe the law of motion of parts of a rigid body. There is no *necessity*, however convenient it may be, to introduce the notion of force; the other bodies in the universe are a sufficient cause for motion of each, without postulating an entity, force. The principal reason for introducing this notion was to account for a body acting where it was not; force was invented to get over this; the body produced force, and this force existed where the body did not, and there acted on other bodies. This whole difficulty seems, however, to be partly due to want of distinct ideas connected with the question of where a body *is*. We are so accustomed to consider a body as having a definite boundary, that we think there is a definite boundary in reality. All we know of the atoms and molecules, however, would lead

us to conclude that round the centre of each there is a very complexly structured region which may or may not change abruptly in structure, but which often extends to considerable distances from the atom, so that it is practically impossible to state absolutely where the atom ends and where the empty space begins. With this view of matter there is no serious reason why we may not rightly consider each atom as existing everywhere that it acts, that is, throughout the whole of space, for its action in causing gravitational accelerations exists, so far as we know, throughout space. A view of this kind entirely gets over any difficulty of a body acting where it is not; for all bodies *are* everywhere, and if we consider matter to be the cause of motion of other matter, there seems no very imperious necessity for imagining another cause which we call force.

There are two assumptions that Hertz makes which he considers can only be proved by their success. One is that all the connexions in nature can be represented by linear differential equations. There are plenty of cases imaginable in which this would not be true, as, for example, connexions depending on the curvature of the path. The other assumption is that forces can be represented by force functions. This, again, may not be a complete representation of nature.

Following this introduction comes the book itself, which is divided into two parts. The first part is purely kinematical, the second deals with the deductions from Hertz's fundamental postulate of motion in the straightest possible path.

The first part begins by explaining what is meant by the path of a system of points. To get at this we calculate the mean square of the displacements of a system of points when they are displaced: the square root of this, Hertz calls the displacement of the system of points. If there is a mass at each point, then the displacement of the system is the square root of the mean squares of the displacements of the points, each multiplied by the mass at it. Thus, if s be the displace-

ment of the system, and s_1, s_2, etc., the displacements of each point of masses m_1, m_2, etc., then

$$(m_1 + m_2 + \dots.)\, s^2 = m_1 s_1^2 + m_2 s_2^2 + \dots.$$

By taking s_1, s_2, etc., as the displacements in the element of time, we evidently get a similar expression for the velocity of the system, and for its acceleration. The mean square of the velocity of the parts of a system is well known in connexion with the principle of least action. Further than this, however, Hertz defines the angle between two displacements. This is defined by the equation

$$(m_1 + m_2 + \dots.)\, ss' \cos\varepsilon = (m_1 s_1 s'_1 \cos a_1 + m_2 s_2 s'_2 \cos a_2 + \dots.)$$

s and s' being the two displacements of the system as calculated above, and s_1, s'_1, etc., the two displacements of each point and a_1, a_2, the angles between these latter; then ε is the angle between s and s'. Hertz remarks that these can all be very interestingly expressed in terms of space of multiple dimensions, in which analytical diagrams are supposed to be drawn. This, however, represents the real by the unattainable. There follow, then, several chapters expressing these displacements in terms of various systems of coordinates, and discussions as to the conditions that the connexions of a system should fulfil in order that they may be represented by equations not involving differentials. The curvature of the path is here studied. It is defined as $c = \frac{d\varepsilon}{ds}$, and from this it follows that, representing

$$\frac{d^2x}{ds^2} \text{ by } x'', \text{ etc.}$$

$$(m_1 + m_2 + \dots.)\, c^2 = \Sigma_1^n m_1 (x''^2_1 + y''^2_1 + z''^2_1).$$

The problem then of making the path of the system straightest, is to make c a minimum consistently with the connexions of the system. Now, in accordance with his assumption that the connexions of the system are linear differential equations of the form

$$\Sigma_1^n (P_1 x'_1 + Q_1 y'_1 + R_1 z'_1) = 0,$$

whose differentiation gives

$$\Sigma_1^n P_1 x''_1 + \Sigma_1^n \Sigma_1^n \frac{dP_1}{dx_2} x'_1 x'_2 + \ldots = 0,$$

we are to determine the minimum value of

$$c^2 = \Sigma_1^n \frac{m_1}{m} (x''^2_1 + y''^2_1 + z''^2_1),$$

where

$$m = m_1 + m_2 + \ldots .$$

In determining the variations of these, we must recollect that the positions and directions of displacement, *i. e.* the first differentials of the system, are supposed given, and that it is *only* the second differentials that can be varied in order to make c a minimum. Introducing then, a system of indeterminate coefficient λ, μ, etc., corresponding to the equations of condition, we evidently get a system of equations of the form

$$\frac{m_1}{m} x''_1 + \Sigma_1^n P_1 \lambda = 0,$$

which are sufficient to determine the second differentials required.

From this form of result one can see how the ordinary equations of motion are derivable from the conception of the straightest path, and how, when dealing with part of a system, these indeterminate coefficients introduce what are equivalent to forces. This method of deducing the equations of motion lends itself particularly well to the deduction of the principles of least action, and the other general methods in dynamics. So far, he deals with free systems subject only to internal constraints. It is where he investigates how to deal with parts of systems that he requires to consider the nature of the constraints joining one part to another. For this purpose he defines two systems as coupled when coordinates can be so chosen that one or more of them are the same for both systems. Force is then defined as the action one system has on another. Now, when a coordinate is the same for two systems, one of the equations of condition is $p = p'$, p and p' being coordinates of the coupled systems; and

for this equation the coefficient P becomes the same in the two systems, except as to sign, so that the equations of motion involve the indeterminate coefficient λ corresponding to this equation equally with reference to each system. It is thus that the equality of action and reaction appears, being thus bound up with the constant equality of the common coordinate. This seems to be where the assumption that the connexions are rigid is introduced. When rigid bodies act upon one another by non-slipping contact, certainly the coordinates of the point of contact are common to the two systems. It is also quite evident that if we assume rigid bodies acting upon one another by contact only, we can have no potential energy, and all *necessity* for talking about the forces disappears. In Hertz's system there are no forces like Newton's acting between bodies which have no common coordinate, like the Earth and the Sun. We would have to invent connexions to explain the motion before we could be certain that action and reaction are equal in this case.

The proof of the principle of virtual velocities by substituting for the forces between parts of a system a number of pulleys which produce the same effects, is quite analogous to Hertz's supposition that the actual connexions are by rigid bodies. It is not, however, liable to the objection that the connexions may become tangled, for it is only applied to the case of infinitesimal virtual displacements, while Hertz postulates the possibility of his connexions existing as the real ones for all time, and throughout all finite displacements of the system.

The work considers many other matters, and shows how all the general methods in dynamics are deducible from his fundamental postulate of the straightest path. It includes discussions on how best to deal with systems whose connexions do not involve differentials, how to treat cyclical coordinates, and many other matters. It is most philosophical and condensed, and gives one of the most—if not the most—philosophical presentations of dynamics that has been published. It is worthy of its author: what more can be said?

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ON SOME CONSIDERATIONS SHOWING THAT MAXWELL'S THEOREM OF THE EQUAL PARTITION OF ENERGY AMONG THE DEGREES OF FREEDOM OF ATOMS IS NOT INCONSISTENT WITH THE VARIOUS INTERNAL MOVEMENTS EXHIBITED BY THE SPECTRA OF GASES.

[From *Nature*, March 7, 1895.]

IT has been generally held that a sufficient freedom of internal motion in an atom, to explain the spectra of gases, proved that the theorem as to equal partition of energy among all degrees of freedom could not hold; and various suggestions have been made as to why the *proof*, as given by Maxwell, Boltzmann, and others, fails in this case. Prof. Schuster has suggested that the numerous lines need not involve the same number of degrees of freedom, as it is possible that there may be connexions between them such that one or two coordinates would define a motion which when analyzed into its Fourier components, as is done by a grating or prism, would produce a very complex system of lines. However, even one degree of internal freedom would interfere very seriously with the observed value of the ratio of specific heats; and the object of this note is to explain how this difficulty may be surmounted without supposing that the theorem as to equal partition of energy is untrue, for it is not by any means disproved because a certain form of proof fails in certain cases.

It has been long held that the motion of the electrons on neighbouring atoms is very much controlled by the ether between them. The wave-length of light is generally many

times as great as the molecular distances, so that the ether is a practically rigid connector between neighbouring electrons. Suppose now, as a particular example, that 10^6 atoms are, in this sense, and so far as the motion of electrons is concerned, within one anothers' control. In this case the motion of these 10^6 electrons might be defined by means of, say, three coordinates. Hence, if the atoms were spheres, there would be 3×10^6 degrees of freedom plus these three degrees defining the motions of all the electrons. Now, if the total energy be equally distributed among all these degrees of freedom, each atom will only have its share of the electromotions, and its energy of external motion will only be diminished by 3×10^{-6}th part owing to the existence of the internal motion of its electrons. I need hardly say that our methods of calorimetry are by no means sufficiently delicate to detect anything of this kind. There might be a thousand such internal degrees of freedom, and yet the ratio of specific heats would agree with observation.

There is some analogy between this suggestion and the case of a sphere moving in a liquid. The presence of the liquid, although apparently endowed with an infinite number of degrees of freedom, does not really increase the degrees of freedom at all, because its motion is entirely defined by the motion of the sphere. In a somewhat similar manner, I would suggest that the presence of the million electrons does not sensibly increase the degrees of freedom of motion of the million atoms, as all their motions may be defined in terms of the motion of a few of them. That the ether would so control the motions of electrons seems almost certain from what we know of the rapidity with which electromagnetic actions are transmitted by it, showing how completely it behaves in respect of them as a system of rigid connexions.

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HELMHOLTZ MEMORIAL LECTURE

[Delivered before the Chemical Society of London. *Trans. Chem. Soc.* January, 1896.]

"And Nature, that old nurse, took
The child upon her knee,
Saying, 'Here is a story book
Thy father has written for thee.

"'Come wander with me,' she said,
'In the regions yet untrod,
And read what is still unread,
In the manuscripts of God.'

"And he wandered away and away
With Nature, the dear old nurse,
Who sang to him night and day
The rhymes of the Universe."

LONGFELLOW.

WHEN standing in Chamounix, beneath Mont Blanc, we are no doubt impressed with its majesty, and oppressed with a sense of our insignificance; but we must remove to a greater distance if we would judge properly of the relative importance of the giant peaks around us, if we would see *the* great mountain towering above its neighbours, the heaven-supplied source of those glorious glaciers that in Chamounix seemed to spring from the lesser heights around. We are now only a little more than a year after Helmholtz's death, and hardly sufficiently removed from his times, from the immediate presence of his contemporaries, from the personal acquaintance of so many great minds, for us to judge with perfect confidence of

the relative importance of the giant intellects around us. His contemporaries, no doubt, valued "gentle Shakespeare, fancy's child," as they valued Ben Jonson or Marlow; but it was reserved for succeeding generations to appreciate how immeasurably this woodland warbler outstripped these in majesty of intellect. That Helmholtz has been one of the great scientific intellects of his time is already abundantly evident. We can, however, hardly judge what his exact position as a founder of the doctrine of conservation of energy, of the theory of vortex motion, of the principles of sight and hearing, is in relation to those other great scientific intellects,—Faraday, Maxwell, and Hertz, who have laid on a new foundation our knowledge of the all-pervading ether,—or Jenner and Pasteur, who have created a rational foundation for medical and surgical treatment, and originated the conceptions by means of which we hope some day to learn the structure of matter,—or Darwin, who has done more than any other, since Copernicus and Newton, to revolutionise man's conception of the world around him, and of his place in Nature.

It is remarkable how many great men have attributed their peculiar gifts to the peculiarities of their family life. Helmholtz seems to have considered the drudgery of school as little better than a necessary evil. What he liked, that entered into his life; what he disliked, was hardly assimilated at all. He puts it—"What a man does easily he usually does willingly." I would rather say—"What a man does willingly he usually does easily." It is the taste that creates the will to do, and that makes the hard work easy. His love for nature was satisfied by learning geometry from wooden blocks, by walks in the country with his father, by experiments with acids on his mother's linen, by making telescopes with spectacle glasses, by studying books on physics in his father's library. Thus was nursed that impulse to dominate the actual world by acquiring an understanding of it, which became the passion of his existence, and guided his whole life. Thus his real life seems to have been nourished by things quite outside his school experiences,

and he seems hardly to have looked upon educational institutions as truly educational till, in the German University, he had scope for expanding in the directions for which his nature was specially fitted. Like an ivy plant in an herbaceous border; clipped here and there to suit its surroundings, it never expands freely, nor does itself justice, nor produces really healthy growth, till transplanted to the neighbourhood of the wall over which its nature is specially fitted to make it grow, and where it develops into its perfect ideal. This passion to dominate nature drove young Helmholtz to attack great problems. Young men are favourably circumstanced for doing this. Many try, few succeed. Hope, enthusiasm, energy are theirs. Unencumbered by old habits of thought their minds develop new roads of attack. The development of habits is necessary for the individual, and hence for the race, but it stops development along new lines. Hence the necessity for kindly death, that new minds may grow, that mankind may improve. To young Helmholtz, the then being discovered principle of the conservation of energy, seemed almost self-evident. He thought that all the scientific world was as satisfied as himself, except a few medical and biological students, whose studies had kept them out of the flow of physical science. For the sake of these he wrote a paper on the Conservation of Energy, and then it appeared that he was a pioneer. Physical science students had not grasped the generalizations they were engaged on. This young master mind was required to clear the new path, to call the attention of those engaged in removing some immediately present obstacle to the immense prospect around them, to show them the country they had invaded, and hoist the flag of science in the conquered territory.

To Baconian England it seems strange to be told that physicists had to contend against Hegelianism in Germany. That preconceived notions of what ought to be may obscure mankind's vision of what is, in both scientific and religious circles, is what we all expect; but that what we in England would call metaphysical views of phenomena should obstruct diffusion

of truth, shows a seriousness about metaphysics in Germany that it would be hard to parallel in England. Britain has produced one of the most original and brilliant metaphysicians since Plato, the great and good Bishop Berkeley; but this father of Hume, who startled Kant from his scholastic dreaming to create all modern metaphysics, has never been taken seriously by his fellow-countrymen. Metaphysics is to most of them inanity, and we consequently wonder, when we see metaphysical theories taken so seriously elsewhere as to produce effects similar to those produced here by authority or by religion.

Great problems attracted Helmholtz's attention: those of spontaneous generation and of the source of energy in muscular contractions. But after a little work on these, his duties, as lecturer in Königsberg on general pathology and physiology, led him to invent the ophthalmoscope, which raised him so much in the estimation of the world and of his colleagues, that he was thenceforth able to devote his time and energies to the subjects he liked best himself, or to such as he could not get others to do for him. To his assistants and students he left the carrying out of investigations whose methods and ends were so far defined that a student or assistant could carry them out with occasional direction from headquarters, while he himself engaged in investigations that might lead to no result, and the methods of attacking which were as yet unknown. How fortunate for mankind that this great intellect had this assistance, that the endowments of German universities should be available for providing such assistance, instead of being frittered away in providing scholarships for wealthy schoolboys to bribe them to devote unwilling energies to the culture of their minds, an end that would be much better attained by the cheaper method of inflicting heavy fines or other such well deserved punishments on those who wilfully and wickedly throw away the great opportunities of improvement presented for their use. Helmholtz thus obtained time for continuous thought, time to wait for discoveries, time to prepare the ground of thought by

familiarity with the problem in all its aspects, time for the evolution of all sorts and kinds of hypotheses, to be rejected one by one, till by a process of natural selection the teeming brain at last saw the true solution break forth like the Sun from behind the clouds that this Sun has all along illuminated, and each bright spot of which had hitherto held out hopes of success. As the natural selection of animals gradually built up mankind from an undifferentiated jelly, so the conscious selection of a genius builds up the great idea from the germs of thought. Continuity and variety are in each case the conditions for rapid progress, and in the brain workshop of a Helmholtz we see the indefatigable workings of a superabundant productiveness bringing forth innumerable varieties for selection: the brain action is natural, too. To Helmholtz the results of his work are the "natural" outcome of what went before. Is there any real difference between the productions of nature and the productions of intellectual activity?

To oneself, no doubt, the selection each time is for the sake of some ulterior end, for the attaining of some object, due to a final cause, but to another observer the brain action must all be explicable as due to efficient causes. These latter do not include the former; rather each makes the other possible. Can it be otherwise in that teaming creation of which we are observers, in which we only see efficient causes, for we are only observers, and not feelers of the thoughts of nature?

And which of all the children of his intellect did Helmholtz value most? Not the ophthalmoscope that took him a week to get to work, nor his investigations on the physiology of sight and hearing that took him years to work out in their innumerable details, nor those theories of chemical and electrical action that occupied his later years. No, it was his victories over mathematico-physical problems of fluid motion, vortex motion and discontinuous motion, that he seems to have looked upon as his greatest triumphs. These seem to be the problems that he speaks of as having given him such months and years of thought to find a solution of; that he attacked this way and that, and

at last solved, and having solved, saw then a broad and easy path to lead others by; a path that was obvious enough when seen, but had yet eluded his grasp, till after years of devious wandering he reached the goal, and then discovered the direct route. Their very simplicity now makes it almost impossible for those who follow to see in these works the value Helmholtz attaches to them. Great and magnificently general as they are, laying as they do a possible, nay, probable, foundation for a dynamical explanation of all natural phenomena, they seem now so obvious that, like Galileo's law of the motion of falling bodies which laid the foundations of all dynamics, they are almost taken for granted as a natural possession of the human mind. We are inclined rather to think our predecessors must have been blind, not to see such obvious things, than to credit the discoverers with preternatural insight.

To the Chemical Society the question of most importance is how Helmholtz's work bears on chemical theories. Unfortunately, there is very little really known about this in respect of vortex motion. Lord Kelvin has made the brilliant suggestion that atoms are vortex rings, and upon this all subsequent guesses have been founded; for our knowledge in this direction is very little better than guesses. Helmholtz had shown that vortices in a perfect liquid could neither create nor destroy one another. Once created, a vortex ring is permanent, like an atom of matter. It can go fast or slow, it can vibrate, it can enter into partnership with others or rebound from them, according to circumstances, but vortex rings cannot destroy one another. They can change in innumerable ways, can be drawn out into thin threads, or gathered up into spherical knots; but one of their properties, called their vorticity, remains indestructibly the same during all these changes. This was Helmholtz's great discovery, and is the foundation for Lord Kelvin's brilliant suggestion that atoms are vortex rings in a perfect liquid. Advances upon this suggestion have been made, though most of these advances have brought to light serious difficulties in the way of accepting the hypothesis in

its primitive simplicity. Something has been done to attack the problem of a medium like a gas of thin vortex rings, and has brought to light two very serious difficulties. The more energy we give to the rings the greater is their inertia, and the slower they move. Now it is generally believed that the inertia of a body is independent of its temperature. We can be sure that the ratio of the inertia to gravitation is independent of temperature to a very high order of approximation. We may be practically certain that the mean temperature of the Earth is much higher than that of the Moon, and that of Jupiter than of his moons. Now, if the inertia of a body increased with its temperature but its coefficient of gravitation did not, it would lead to very serious discrepancies in the astronomical theory of the motions of the various members of the solar system. An investigation of possible small departures from constancy of coefficient of gravitation would be most interesting in connexion with the question as to the effective inertia of electrons. Departures from constancy of refractive power seem to show that the drag of ether on matter is not exactly proportional to mass, and if any sensible amount of the inertia of matter is due to an ether disturbance accompanying its motion there might be a difference between the inertia of the matters constituting what are probably such differently constituted bodies as the planets. We cannot be at all so sure that both inertia and gravitation do not increase simultaneously with temperature. Very few experiments indeed have been made as to the variation of the weight of a body with temperature. It is difficult to weigh hot bodies accurately, and, in consequence, there does not seem to be any conclusive proof that the weight of a body does not change with its temperature. If it does not do so by a measurable amount the simple vortex-ring theory of matter can hardly be true. As regards the velocity with which vortex rings move, the elementary dynamics of how sound is propagated by a medium like gas, in which the molecules are only for a short part of their time in collisions, acting on one another, and are for by far the greater part of their time engaged in flights

between collisions, proves that the velocity of sound must be proportional to the velocity of flight. It is known that the velocity of sound increases with temperature, and so we are landed on the horns of a dilemma, either the molecules are not thin vortex rings in an otherwise simple liquid, or else when we give heat to a gas we are in some mysterious way taking more energy from it than we give to it. This latter suggestion, if it can really be worked out, would get over the other difficulty as to the variation of inertia with temperature, for the suggestion is based upon the supposition that we have to deal, not with the motion of the ring as a whole, but only of its rotational core. Some people may have rather a preference for this latter horn of the dilemma, but I certainly think that all we know of matter and the ether points to the conclusion that whatever atoms are, there is certainly a complicated structure in the space between the atoms. This is required to explain electromagnetic actions, and consequently atoms are certainly not simple thin vortex rings in an otherwise unmoving and structureless liquid. It has been shown that an intensely moving liquid could propagate disturbances somewhat like light, if there were no diffusion of the motion, and at the last meeting of the British Association we were told of a structure for that motion which would not diffuse.

In this connexion, it may be worth while pointing out that as vortices move of themselves freely and independently of the density of the liquid, there is no necessity for supposing that the density of the liquid is small. In fact, the simple theory would point to the conclusion that its density may be greater than that of platinum. This would require the energy per cubic centimetre of the turbulent motion to which the propagation of light is due to be very large indeed, approaching a million of millions of kilojoules, *i.e.*, it would be equivalent to a million horse-power for 10 hours a day for nearly a month. It has been suggested that an atom is a hole in a vortex-cell ether. The suggestion is ingenious, but is hardly consistent with the separateness of atoms, their ability to combine and separate

again regaining all their primitive properties. So far, no better suggestion has been made than that atoms are regions of vortex motion, and as it seems quite possible that a vortex ring might take up a steady motion of association with a vortex cell, and in moving from place to place may carry energy proportional to the square of its rate of translation, and still more as no one knows with certainty how vortex rings would behave in a structureful medium such as the ether is, I prefer in our ignorance the horn of the dilemma, that holds that atoms are not simple thin vortex rings in an otherwise unmoving liquid.

The theory does more, however, than land us in dilemmas. There is another result of Helmholtz's work which, though it too is full of difficulties, has a sufficiently curious analogy to chemical facts to make it wonderfully possible that there is some connexion between the two. When two parallel vortex threads are near one another, and are both rotating in the same direction, they can take up a steady condition of rotating round one another which is pretty stable. Three vortex threads can do the same, taking up positions at the corners of a triangle. Four, five, and six can behave likewise, but if we try to make seven vortices rotate round one another the condition becomes unstable. That there are unstable states of this kind seems certain, but there is also no doubt that any number of vortices could exist as a stable system, thick vortices being in fact collections of vortex filaments. The chemical analogy is, however, between these systems of vortices that become unstable when more than six are involved, and the chemical fact that no element has ever been required to possess more than six monovalent elements simply connected with it. Now, though the connexion is undoubtedly far-fetched, and though the suggestion does not seem to explain why three or more hydrogen atoms should not be pretty fond of revolving round one another in a happy family, yet there is undoubtedly something very striking in the numerical coincidence between the number of bonds required for chemical combinations and the number of vortices that can be absorbed into a single system of this kind. Pending any other dynamical suggestion as to

why chemical bonds are limited in numbers, there seems sufficient grounds from this coincidence for keeping the matter prominently in view, in hopes that further light may be thrown on these fundamental questions. The other directions in which one might look for numerical coincidences of this kind are in the finite numbers of stable positions of magnets, in the finite numbers of the regular solids, and of the possible crystalline systems. Considering the success that has attended the development of the analogies founded on the geometry of the tetrahedron in the case of carbon, it is certainly well worth while developing corresponding solid geometrical analogies in respect of other elements. Considering that a vortex ring is in many respects an area over which sources are distributed on one side and sinks on the other, and that sources and sinks behave in some respects like positive and negative electrified points, there seems some possibility that vortices may in some respects behave like polar bodies. From what we know of nature, a kinetic analogy is always to be preferred to a statical one, being more probably *like* the truth. If it is required to connect the geometry of the tetrahedron as regards the asymmetric carbon bound molecules with the vortex-ring theory of matter, we must proceed somewhat as follows. It appears at first sight as if it would not suffice to arrange the four vortex filaments that rotate with the carbon simply at four of the corners of a pentagon to make a system competent to circulate along with the carbon filament at its fifth corner. It is well known that four points in a plane cannot represent the atomic bonds of carbon. One of them must be out of the plane of the other three in order that the rotational properties of many carbon compounds may be represented. In the case of the vortex filaments a corresponding result might be obtained by supposing the attached filaments to be wound corkscrew fashion round the carbon filament. Such corkscrew filaments are, no doubt, possible, but, unless vortex filaments can cross one another, which is certainly not generally possible, no free vortex ring could be wound corkscrew-wise round another. The same result might follow by supposing

that there are right-handed and left-handed carbon atoms. Though such might undoubtedly exist, this seems a very improbable explanation of the rotational light phenomena in view of the possibility of changing one of these isomeric bodies into the other by a moderate rise of temperature. We must, then, conclude that the difference between the right- and left-hand distributions of filaments must be a difference of their order of distribution round the pentagon *taken in conjunction with* the circulation of filaments round one another. If we suppose four filaments, *a*, *b*, *c*, *d*, arranged in this order when we go in the direction of circulation, we can have a complementary system arranged in the order *d*, *c*, *b*, *a*, taken in the direction of circulation, and these two systems differ in the same sort of way as a right- and left-handed screw. In all these arrangements there would be a greater facility of interchange between *a* and *b*, *i.e.*, between neighbouring filaments, than between *a*, *c* or *b*, *d*, so that in addition to this right- and left-handed isomerism there would be a possible distinction between *a*, *b*, *c*, *d* and *a*, *c*, *b*, *d*. This, so far as I know, has not been observed, and would consequently throw doubt on the suggestion that this vortex analogy is founded on any real likeness. There seem also very great difficulties in extending the analogy to complicated cases, such as the benzene ring, where a carbon filament has to be partly satisfied by one bond of a complex molecule, partly by one bond of another carbon atom, and partly by two bonds of a second carbon atom. Too little, however, is known of the possible combinations of vortex filaments to be at all sure whether six filaments, each with an attendant satellite, could not very well circulate round one another in a stable group. In this case, as in the former one, the direction of circulation would bring us the two isomeric distributions of the satellites round the system that is at present represented by the alternate double and single bonding.

There are several serious difficulties from the chemical point of view to the supposition that these combinations of thin vortex rings are really like what is actually taking place. The

possibility of a permanent combination of rings has only been proved possible when the strengths of the components are equal. Now this is quite out of harmony with known chemical phenomena. Compare, for instance, the two similar bodies water and cetylalcohol. Their chemical compositions, H·O·H and $C_{16}H_{33}$·O·H, and behaviour show that the complex system $C_{16}H_{33}$ goes in and out of combination in just the same sort of way as the simple atom H. Now a vortex ring, $C_{16}H_{33}$, consisting of thin filaments, would according to theory be 65 times as strong as the simple H ring, and consequently would not behave as a whole in at all the same way as this H ring. If it is really necessary that the components of a system must be of the same strength, the consideration of the various alcohols is fatal to the theory that chemical combination is at all like this association of vortex rings. However, the proof that equality of strength is necessary in order to make this association permanent seems to me inconclusive, and so there may yet be something important in the analogy.

Another serious difficulty is as to the explanation of the enormous differences of mass of the atoms. Mercury, for instance, is 200 times as massive as hydrogen. Now, according to the hypothesis that has been proposed, the strength of the mercury ring is the same as that of the hydrogen one, and the only way in which it can have this enormous mass is by its having a radius 14 times as great. If the average volume occupied by an atom be considered as proportional to the sphere upon which the ring would lie as a great circle, it would follow that the volume of a mercury atom would be something like 2800 times as great as that of a hydrogen atom. This does not seem at all in accordance with what we know of these bodies. This difficulty would be largely surmounted if we could adopt the proposal already mentioned of supposing that when dealing with vortex rings we have to deal with the momentum of the core of the ring, and not with that of the whole motion involved. We might, then, suppose that massive rings had thick cores with a slow rotation, so as to have the same strength

but a greater momentum than the thinner rings, even though the radius of the rings as a whole differed only slightly. The theory of nearly spherical and worm-like vortices would lead to a somewhat similar solution, but then the theory of their association into systems has not been at all fully worked out, and we cannot say whether it does or does not lead to results such as are known to hold in chemical combination.

A further difficulty from the chemical point of view is that carbon should be a diad, and only by becoming doubled upon itself in a way that has never been proved to be possible for a vortex ring can it become a tetrad. Similarly, nitrogen becomes essentially a monad, and there seems to be no explanation of N_2O_5. The suggested doubling by which carbon becomes a tetrad would produce two allotropic forms of tetrad carbon, one right-handed and the other left-handed, and there is no evidence for this. In fact the whole system of atomicity as evolved from the theory of the association of thin vortex rings is sadly at variance with that derived from the study of chemical facts.

This should induce study of other forms of vortex motion—study of thick rings and of spherical and worm vortices. There are several ways in which these latter are not subject to the same objections as thin ring vortices. They in some cases increase in velocity when energy is given to them, so that the objections depending on the velocity of sound increasing with temperature would not apply. The change of inertia with temperature would still exist. They could apparently swallow one another up, so that something analogous to chemical combination could exist, but too little is known about this to know whether the theory of their association would be less open to objection than that of thin rings. At the same time I expect much more from the investigation of the properties of a complex medium which would be capable of producing electromagnetic actions. Any suggestion as to the nature of atoms that does not explain electrolysis cannot be more than an analogy. There is some hope that a liquid full of energetic motion could explain

electromagnetic actions, and a theory of electromagnetic actions depending entirely on the actions of electrons associated with atoms has been worked into it. The suggestion that electrons have an individual existence is undoubtedly tempting, but it is worth while keeping constantly in view the possibility that their constancy of quantity is connected with a constancy of structure of the ether rather than with any individual existence of each electron. This whole subject is so tentative that it is very doubtful indeed whether any real use could be made of these kinetic analogies at all comparable with the use that has been made of the statical analogy of carbon to a tetrahedron. Although kinetic analogies are certainly to be preferred to statical ones, yet our knowledge of the mechanism involved is still so very imperfect that, as in the case of the elasticity of solids and even of gases, we are perforce driven to think in statical analogies, and, though we know that the energy is kinetic, to deal with it as if it were potential. There is therefore no sufficient reason at present for not using corresponding statical analogies to represent chemical equilibrium, even though we may be pretty sure that we are not dealing with the matter in the manner which is absolutely like what is actually taking place.

The possibility of right-handed and left-handed vortex atoms has been mentioned. There are, in addition to the doubling of the atom on itself already mentioned, two ways in which these might exist. There might, in the first place, be a sort of temporary right- or left-handedness produced by a corkscrew wave motion being propagated round the vortex. Such a vibration could hardly subsist permanently amongst the clash of molecules, but it may be worth keeping in view. There might, in the second place, be a circulation of liquid inside the vortex core either in the right- or left-hand direction round the line of motion of the vortex ring.* This would be a permanent quality of each atom, one that no interactions of vortices could either produce or destroy. But it is a quality that might be a

[* Developed mathematically by W. M. Hicks, *Phil. Trans.* A, February, 1898.]

very serious one for the vortex ring itself if ever it got drawn out beyond a certain length of circumference, for it might then make the circular shape quite unstable, and the result would probably be a vortex tangle instead of a vortex ring. Such a vortex tangle, if it could be kept within bounds, might do very well for an atom and may be better than a vortex ring, but the theory of such a tangle is beyond me, and I am afraid there would be a tendency for it to spread itself throughout space instead of confining itself to one place. May be it is by just such a destruction of vortex-ring atoms that the ether has been made; and it may be that from time to time in the immensity of space these tangles undo and reform the atoms, thus rebuilding the universe out of its fragments and utilising the chance, by which the laws of thermodynamics prophesy the end, to prevent this very consummation. Anyway there seems plenty of scope for speculations founded on Helmholtz's discovery of the law of vortex motion, though I fear that until some genius directs our attention aright it can give little or no help to the chemist in his investigations.

One of the most interesting matters of a physiochemical character that Helmholtz worked at was the application of thermodynamics to chemistry. The application is, as yet, very limited. Comparatively few chemical processes can be brought through a complete cycle of operations, and still fewer are completely reversible. Unless these two conditions can be shown clearly to be applicable, we can only apply the principle of the conservation of energy to the process in making quantitative calculations, and the second law of thermodynamics can give us very little quantitative information. There is, however, one important class of cases of chemical action which is reversible and to which we can consequently apply thermodynamics, namely that of a large number of electrolytic phenomena. Helmholtz has done a good deal in this direction, but a great deal still requires to be done both experimentally and in working out all that can be deduced from the experiments. In his method of treating the subject, he introduced a certain quantity

which he named the free energy. This name is rather misleading. One would naturally conclude that it was connected with the proportion of internal energy that can be transformed into work by means of an engine; and it is connected with this, but in such a roundabout way, that anybody might easily make mistakes about it. When the internal energy is described as being divisible into two parts, one of which is bound energy and the other free, one would naturally conclude that no matter what one did, one of them could never be turned into work, while the other could, and that there was some difference in the nature of the energy in the body that made this distinction of quality. Now the internal energy in the body is really homogeneous, and we cannot separate it out into parts having different qualities in this way. As a matter of fact, changes of temperature change free energy into bound energy and *vice versâ*; and free energy is the energy turned into work during isothermal changes, and is not derived wholly in general from the internal energy. In the case of a gas, for instance, there is during isothermal expansion practically no change in the internal energy, while free energy is being expended on work. Hence the view that the internal energy can be divided into two parts, one bound and the other free, is a very forced view of what is really taking place. It is worth mentioning this because the name is seductive, and those who are not familiar with a subject are very liable to be misled by the seductive appearance of simplicity conferred by particular names. Of course this does not in the least detract from the value of the function itself, which has been used under a variety of names by various investigators, and has proved in Helmholtz's hands a valuable means of deducing all that can be learned from measurements on electrolytic cells and from their alteration with concentration, temperature, etc. The complete determination of the thermo-dynamic properties of even one substance is not known. We know hardly anything about the changes of the specific heats of bodies with temperature and pressure. A few isolated attempts have been made to determine them, but

in general our knowledge of the thermodynamic functions of bodies is extremely meagre even as regards temperature and pressure, and almost *nil* as regards chemical changes of composition. This is a direction in which much more remains to be done by the co-operation of the chemist and the mathematical physicist. Many other applications of thermodynamics to chemical actions have been made since Helmholtz's time. By the use of so-called semipermeable diaphragms the process of diffusion of a salt in solution can be made reversible at least ideally. The theory of how semipermeable diaphragms act is not of much importance in these applications of thermodynamics, unless indeed they are necessarily destroyed by their own action. If this is the case they do not make the processes involved reversible, and all the thermodynamic investigations that have been founded on the supposition that they are themselves permanent and unaltered are invalid. All that is necessary for the thermodynamical theory is that a semipermeable diaphragm is possible; the fact that actual diaphragms are by no means absolutely semipermeable is an objection no doubt to the observations founded on what they do and is a serious difficulty in comparing observation with calculation, but it is no more an objection to the calculation of what would take place if a semipermeable diaphragm is possible than the fact that even ice and air offer resistance is an objection to the prediction of what would occur if a stone were sliding over a perfectly smooth surface, by calculation from the known principles of dynamics.

Very little is known about the theory of semipermeable membranes. It is generally assumed that there are no heat effects depending upon their presence, but the corresponding mistake in neglecting the heat effects that occur when currents of electricity enter and leave a liquid led to serious errors in the theory of galvanic cells, and until the theory and practice of semipermeable membranes agree more closely than they do at present, it is worth while looking for something of this kind. They are often spoken of as if they were merely some kind of

molecular sieve. They seem really much more analogous to Graham's second class of membrane which only permit the passage of gases which dissolve in the membrane, so that their behaviour is quite different from that of simple sieves like earthenware, etc., for which the laws of diffusion are so very simple. When we come to deal with really molecular magnitudes, it is impossible, however, to draw a hard and fast line between physical and chemical permeability. One molecule may be able to penetrate amongst others, not so much because it is of the right size to get between them as because it has the right shape to do so. One cannot fit a square bar properly into a round hole. This question of shape is here mentioned merely as an example of one of various peculiarities of a molecule which may enable it to associate itself intimately with others in a way peculiar to itself. Such a peculiarity can in our ignorance only be called by the general term one of its chemical peculiarities. From the peculiarities of organic diaphragms there seems some reason to hope that we may be able to produce a set of judiciously constructed diaphragms each suited to let through molecular groups peculiar to itself, and thus enlarge our means of what may be called chemical filtration. So far we seem to have attained very little farther than to be able to filter water from salts in solution.

At the same time many organic processes seem to require an actual carrying of material by currents such as we see in the living cells of plants. By such currents materials can be taken out of dilute solutions and then transported to places where they can, under new circumstances, be given up to strong solutions. Actions of this kind, of course, like the carrying of heat from low to high temperatures, require expenditure of energy. In living organisms we have many sources for this energy; and currents of the kind can easily be produced by superficial actions like the motion of camphor on water, which might continue till all the camphor was used up if clean water were constantly supplied.

In applying thermodynamics to chemical investigations there are some serious pitfalls into which investigators have

fallen. In the application of the law of the conservation of energy there is great risk of assuming that the heat supplied is equivalent to the work done, without bringing the system through a cycle. A very serious mistake of this kind has been recently published as a proof that osmotic pressure is proportional to the absolute temperature. This is very possibly true, although the experimental evidence is by no means for it, but rather the contrary; yet the proof given, which assumes that the heat supplied is all used in doing osmotic work, is entirely inconclusive. A complementary mistake is to neglect the external work entirely when it is small in the cases in the mind of the calculator, and to assume that the heat supplied is all spent in changing the internal energy. This may lead to very simple laws, and to ones that are approximately true in a great many cases, but a proof founded on any such neglect and which does not clearly point out the degree of approximation attained is utterly unsound. Yet a mistake of this very kind occurs within the first seven pages of a very valuable treatise on theoretical chemistry, so that it can hardly be considered unnecessary to warn others of such mistakes. Another pitfall is to neglect the necessity for operations being reversible. Theories as to explosions in guns and gas engines have been propounded and published in which this is neglected. The operations in both these cases are not reversed in the engines considered, and so the generally applicable second law of thermodynamics cannot be applied to them accurately. A similar mistake has been made in calculating the efficiency of engines when the feed supply, *i. e.* the return process, is worked by a separate engine. If this method were used in an air engine, it could easily be arranged to give an efficiency much greater than what is generally called the theoretically possible efficiency. In a recently published work, it is stated, in connexion with a thermodynamic investigation, that one of the processes involved is the mixture of water and salt solution, an irreversible operation unless by means of a semipermeable diaphragm, which is not mentioned. There are several proofs in vogue depending on the supposed incapacity of a system to

cool itself below its surroundings. These sometimes neglect the fact that for this to be impossible the operations must constitute a cycle. By evaporation, bodies cool themselves below their surroundings, and in some cases where vapour pressures of different materials are considered in two limbs of a closed vessel, this possibility is overlooked, and at the same time the possibility is overlooked of both the limbs getting coated with the same material by distillation of a small quantity from one to the other. The failure of these proofs does not of course involve the falsehood of the thing proved, but it shows how careful those who apply physical principles should be, to be quite familiar with the subject considered. It is as risky for a chemist to apply mathematics as for a mathematician to lecture to chemists: we should work in co-operation.

In the case of calculating the connexion between osmotic pressure and the lowering of boiling point by supposing that the level of the solution is raised above that of the pure water by means of a semi-permeable membrane, an extension of the theorem may be obtained by supposing the membrane sunk to different depths in the water, when an interesting theorem connecting the osmotic pressure with the total pressure in a liquid can be deduced.

Notwithstanding all these pitfalls and mistakes, great advances have been due to the applications of thermodynamics to chemistry, at which Helmholtz worked so fruitfully.

Along lines closely allied to those of thermodynamics, there are an immense number of chemico-physical investigations waiting to be attacked. A good deal is known about the way in which variation of composition affects capillarity for instance, and from this it is possible to calculate how capillarity ought to affect chemical composition, but there is very little indeed known as to whether it behaves as it ought. There are an enormous number of similarly reciprocally related phenomena which are connected with one another in a manner analogous to the way in which, for instance, freezing point and pressure are reciprocally related, and which demand attention.

In all such work, the co-operation of chemists and mathematical physicists is eminently desirable. We cannot often expect the two to be united in one person, with the energy, and time at his disposal to do double work.

Helmholtz laid the foundation for considerable advances in our knowledge of electrolysis. By a very carefully conducted series of experiments he distinguished between the diffusion currents and electrolytic currents. The former seem to be the only representatives of metallic conduction in an electrolyte. For the latter he verified with very great accuracy Faraday's law of electrolysis and showed that the chemical forces are balanced by electrical forces, and that so far there was no reason for doubting that the chemical forces are electrical. This was Faraday's position, and is largely justified by subsequent investigation. At the same time there seems no doubt but that chemical phenomena are much more complex than simple electrolysis as we know it. There are too many irreversible chemical actions for us to be able with certainty to co-ordinate them with simple reversible electrolysis such as Helmholtz specially studied. There are too many good reasons for being sure that other forces exist than merely electrical ones for us to be at all certain that it is only these latter which are of importance in chemical changes. We can balance gravitation by the elastic force of a spring, or by the centrifugal force of a pair of governor balls, but that can hardly *prove* conclusively that all these are of the same kind. We can balance electrical forces against chemical forces, but this does not prove conclusively that they are of the same kind. We know that gravitational forces and magnetic forces must exist between atoms, and though they *may* produce only a very small part of the effects observed, we hardly know enough about them to be perfectly certain that their effects *must* be very small. One of Helmholtz's own theories seems to prove conclusively that there are forces acting which are *not* electrical. Helmholtz worked fruitfully at a theory of electrical diffusion through fine tubes, which assumes that there is a double electrical layer on

the surface of contact of the liquid and solid, caused by a differential attraction of the two materials for positive and negative electricities. Now *this* attraction cannot be a force varying inversely as the square of the distance, for if so, it would merely neutralize the action of the electricity it attracted. A layer of positive electricity attracting one of negative electricity would just attract enough to neutralize all force on the layer; and Helmholtz, in assuming that nevertheless the electric force along the tube due to the current flowing in it does act on the layer, practically assumes that the attraction of the liquid for electricity is *not* an electrical attraction but is a specific attraction of a different kind. If we assume an attraction of this kind can exist between atoms and electricity, we can hardly be justified in denying that atoms can act on atoms in a similar way. If the law of action of these forces be a much higher power than the inverse square, almost all the work done in separating atoms might be done against electrical forces, although at the very small distances involved in atomic electrical dimensions these other forces might be very much greater than the electrical forces at the same distances, and might control chemical combination.

A good deal of labour has been expended on investigating a theory of solids and liquids upon the hypothesis that atoms attract one another with a force varying inversely as the fourth power of their distance apart. Very interesting results have been obtained, many of them independent of the law of action. This particular law of action seems absolutely inadmissible, however, because it is one of the most interesting conclusions from the hypothesis that at one centimetre apart two atoms would have an attraction for one another due to this cause approximately equal to their gravitational attraction. Anything at all approaching this is conclusively negatived by the substantial agreement of the various experiments, some on a large scale and some on a small scale, by which the coefficient of gravitation has been determined. If this suggested attraction existed there would be no sort of agreement between the results

obtained on a large and on a small scale. There seems every reason to think that in the case of atoms we are dealing with phenomena of a much more complicated nature than that of laws of force of this kind. We know almost certainly that in the first place we are dealing with forces which are anyway like electrical and magnetic ones, neither all attractive nor all repulsive, but some one and some the other. From the arrangements of the molecules in crystals, and the tetrahedron analogy of carbon, we may be practically certain that as far as we can deal with the question as a statical problem at all, we must deal with atoms as much more complicated structures than merely centres from which forces radiate uniformly in every direction. There seems every reason to think that atoms have special centres of attraction, and there is even good ground for thinking that the force emanating from each of these centres is directed, *i.e.* is very much greater in some directions than in others. No theory of solids certainly can be satisfactory which overlooks this, and any theory of liquids which overlooks electrolysis must be rather lame.

It must always be recollected that no *statical* theory of a solid or liquid medium is possible with electrical forces alone acting. Such a medium would be essentially unstable. Hence, so far as we can deal with solids and liquids as statical systems, we may be quite certain that other than electrical forces *must be* postulated. From our knowledge of solids, crystals especially, and from the success that has attended the tetrahedron theory of the carbon atom, it seems almost certain that provisionally, at least, we may safely assume that a very large number of the properties of molecules can be deduced from purely statical theories of their structure. Hence we may be quite sure that so far at least as we are working on these provisional lines, we must assume other forces than electrical ones, or any others varying inversely as the square of the distance. Take, for instance, the suggestion that when an electrolyte is subject to electrostatic induction the superficial induced charges are due to a layer of electrified ions upon its surfaces. If there were no

forces other than electrical ones, these ions would fly off the surface like dust. The pressure of the surrounding gas would certainly not prevent this, for a gas never prevents the diffusion of atoms. Hence we must suppose that there are other than electrical forces keeping these ions attached to the liquid. Helmholtz, himself, states as a conclusion of his investigation of the action of reversible electrochemical actions: "A remarkable feature in these processes appears to me to consist in the fact that the attraction of the water to the salt to be dissolved can constitute so great a part of the chemical force acting between the oppositely propelled elements." There seems to be considerable danger that these forces may be neglected. So much advance has been made by assuming that bodies in solution behave in some important respects like the same bodies in the gaseous state, that there has been a serious danger of assuming that the physical conditions are at all like. The dynamical condition of molecules in solution is essentially and utterly different from that of a molecule in a gas. The essential condition for applying any known dynamical theory of gases to calculate their behaviour is that the time during which two molecules are within the sphere of one anothers' action is small compared with the time during which they are apart, and that consequently the chances of three or more molecules being in simultaneous collision is very small. It further follows that the character of one kind of collision has no influence upon the character of the immediately succeeding collision, a condition which is not, however, fulfilled in the succeeding chatters of the chattering collision of elastic solids. Now this essential condition for the application of the dynamics of a gas to molecules in solution is very far indeed from being fulfilled. A molecule is never outside the sphere of action of its neighbours. All we know of the magnitudes of the interspaces between molecules in a liquid shows that each molecule is within the sphere of action of hundreds, probably of thousands, may be of millions, of its neighbours. It is probably jostling about with a path for its centre between its jostles about one

hundredth part as great as its own diameter. If its centre moves at a rate comparable with that of a gaseous molecule, which seems probable from the fact that liquid molecules can escape through the surface into the surrounding gas, the molecule would bang about in its confined space making nearly 10^{14} bangs per second, and these vibrations would be quite comparable with those of heat radiations, and the concomitant vibration of the atom may be a large part of the source of ordinary heat radiation of solids and liquids. In the case of a gas the external virial is large and the internal is small and negligible; in the case of a liquid it is exactly the reverse, the external virial is small and negligible, and the internal large and important. The dynamical theory of a medium composed of molecules which can only jostle about through distances about a hundredth part of their diameter, is evidently essentially different from that of one in which the molecules describe free paths thousands of times the molecular diameter.

The theory of semipermeable diaphragms is in a very doubtful state. They are often spoken of as if they were sieves, and as if the forces between them and the substance in solution were unimportant. A consideration of the relative behaviour of the only really effective semipermeable diaphragm known with that of the solid diaphragms shows that there are some important differences. The surface of a liquid with a non-volatile salt in solution is a perfect semipermeable diaphragm. Water molecules can and do get through quite freely, but molecules of the solvee cannot. Now the presence of these non-volatile molecules in the surface certainly prevents the egress, but either does not prevent the ingress, or possibly facilitates the ingress of the volatile molecules. We know this, because the presence of the solvee reduces the vapour pressure. On the other hand, the whole theory of osmotic pressure assumes that the presence of the body in solution produces no effect, or the same effect on the ingress and egress of the molecules of the solvent. It is, no doubt, a most remarkable thing that osmotic pressure should be even roughly the same as what would be

produced by the molecules of the body in solution if in the gaseous state; but to imply that the dynamical theory of the two is at all the same, or that the dynamical theory of a gas is in any sense an *explanation* of the law of osmotic pressures, is not at all in accordance with what is generally meant by the word "explanation." These osmotic pressures are much more closely connected with Laplace's internal pressure in a liquid which is essentially dependent on the forces between the molecules, than with the pressure of a gas which is essentially almost independent of the forces between the molecules. The existing dynamical theory of a liquid shows that the molecules are kept apart by the mutual jostling of the molecules, and that the dynamical pressure of this jostling must be equivalent to the Laplacian pressure pulling them together. When we make a rough calculation we find that molecules going with velocities approaching those of gaseous molecules, and jostling about in the narrow interstices between molecules that have been already mentioned, would produce pressures comparable with those that have been calculated for the Laplacian pressure. Hence we conclude that although the dynamical conditions in the liquid are so very different from those in a gas, nevertheless, the two methods of calculating *these* internal pressures from the mutual jostlings that keep the molecules apart and from the attractive forces that keep them together, lead to very much the same result, and that the velocity of motion of the centres of the jostling molecules may be of somewhat the same magnitude as that of the same material if it were in the state of a gas. It must be seen from this how the coincidence of these two ways of considering the same question is no matter of accident, but depends on the theory that it is the mutual kinetic jostling of the molecules against one another that keeps them apart. Hence we need not be surprised if the theory of osmotic pressure has a dynamical aspect from the point of view of attractions between the membrane and the water, the water, and the body in solution, and the body in solution, and the membrane. There must be some dynamical reason why the

solvent gets through the membrane while the body in solution does not. It must be due to what may be described as capillary forces between the solid and the molecules of the solvent. There is no sufficient reason for supposing that the membrane acts merely as a sieve. Calculations have been made, founded on the assumption that the membrane acts like a number of fine tubes of molecular dimensions, and that the capillarity of the solution differed from that of the solvent in contact with the walls of these tubes. Upon the supposition that the tubes are of molecular sizes, the observed osmotic pressures can thus be explained by possible capillary differences. That there is a direct relation between the osmotic pressure and capillarity can be shown by causing capillarity to raise a solvent so high in a fine tube that the vapour pressure at its concave upper surface is the same as that of the salt solution, when it is readily seen that this height is that producing osmotic pressure. By means of a fine mesh, that the solution does not wet, the converse experiment is possible, namely, to have the surface of the solution so convex that the vapour pressure near it is the same as near a flat surface of the solvent. These diaphragms, made of an ordinary porous material, with the only known perfect semipermeable surface, namely, the surface of the liquid itself, kept by capillarity in its pores, prove the theoretical possibility of semipermeable diaphragms in all cases where the body in solution is non-volatile, and enable us to discuss the theory of their working more satisfactorily than when dealing with semipermeable diaphragms whose *modus operandi* is uncertain, or, at least, matter of dispute. These perfect semipermeable diaphragms enable us to apply thermodynamics with confidence to calculate osmotic pressure. They enable us to calculate what is taking place at each point, and to be sure that osmotic pressures thus calculated are not dependent on some unknown function of the diaphragm itself. We may, then, be pretty sure that, at least approximately, osmotic pressure is nearly equal to the vapour pressure of the solvee, because that is approximately the result deduced from observations on the

vapour pressure of solutions. At the same time our attention is attracted by the dependence of the whole phenomenon on capillary phenomena. We naturally ask such questions as whether the vapour pressure near a newly-made surface is the same as near an old surface. It is known that the capillarity of a newly formed surface differs in some cases from that of an old one. It is known that there are cases in which the solvee is concentrated into the superficial layer, and there are means for approximately calculating how far this is the case. One would naturally expect that the vapour pressure near this concentrated surface layer might be quite different from that near a newly formed surface layer which had not had time to concentrate the solvee into it. Which of these vapour pressures is the one from which we ought to calculate osmotic pressure?

That osmotic pressure is proportional to the number of active molecules is not particularly remarkable. When small changes are made, the effect is generally proportional to the amount of change. It is, however, very remarkable that the amount of osmotic pressure can be even approximately calculated from the gaseous pressure that would be produced by these same molecules if they alone occupied the *whole space* filled by the solution. The molecules have not got all this space to move about in. They can only move about in the interstices between the other molecules, which we have every reason to think constitute only a very small part of the whole volume. From the rate of diffusion of a salt in solution, we know that its molecules do not go about as if they passed freely through the water molecules. It is evident that the simplest theory of what would be the effect of substituting for a certain number of water molecules a certain number of other molecules must include a consideration of their relative attractions and sizes, and the resultant change in the space available for jostling, to say nothing of such additional complications as direct chemical combination between the two. This latter seems to be *the* complicating cause in electrolytic solutions in which each of the

molecular components of the salt molecule produces its separate effect. If that effect be due to pressures exerted by each component banging about independently of the other, a very moderate amount of independence of the two components on one another would enable them to bang about independently in the extremely narrow interstices within which they are confined. That they can thus independently produce pressure only lends a very moderate amount of confirmation to the theory that these ions are quite independent as pairs. Possibly the mere substitution of an inverse square law of action between the ions for some other law of action between particular points on each, would be quite sufficient to account for the independence observed. This suggestion is founded on the supposition that the action of an ionising solvent is to substitute electrical attractions for the more complex chemical attractions which act between molecules. The difficulty of gaseous thermodynamic theory is to explain how two atoms can be sufficiently firmly fixed together not to have independent motion, rather than to account for this independence. According to this suggestion, the special forces on ions would be electrical ones, and from the known instability of bodies subject to forces varying inversely as the square of the distance, we might naturally expect a continual interchange of partners to be taking place. This, of course, does not assume that there are no other forces in the liquid than electrical ones. I have already called attention to this necessary consequence of any theory that deals with molecules as even approximately statical systems. What it does assume is that in dilute solutions these other forces are so symmetrically distributed that they do not produce stable equilibrium within the liquid. The principal difficulty in explaining how sufficient interchange of partners can take place is in explaining the apparently very great independence required by such results as that the velocities of the ions are functions of themselves alone and independent of those to which they are attached. This difficulty would be largely got over by assuming that in solution the ions are quite as much attached to the

elements of the solvent as to one another, because then the interchanges that take place will be almost always interchanges amongst molecules of the solvent, and only very seldom amongst the very rare molecules of the solvee. It is almost impossible to explain dynamically the supposition that *free* ions with their electrical charges are meandering about in the liquid in a condition that can be at all rightly called dissociated. The term "dissociated" should be confined to a condition in which the components of a molecule are not connected by any chemical bonds at all. In that case they can diffuse freely and independently through porous diaphragms. Hence the possibility of this independent diffusion is the simple and necessary test of the independence of the components which can rightly be called dissociation. In an electrolyte there is *not* this independence. The component ions cannot diffuse independently through porous diaphragms. There is the acknowledged electrical force between the various oppositely charged ions instead of their being *free* from one another. In even very dilute solutions this force is very considerable. If we assume that a negatively charged ion is exactly half way between two positively charged ones, of course the forces on it are balanced, but if we suppose it as little as 1 per cent. nearer one than the other, it becomes subject to a force of 400,000 volts per cm., drawing it to the nearer electron. This at once illustrates the instability of such a condition as that described by saying merely that these charged ions are moving about independently in the liquid. Without some other important actions existing at the same time such a condition is dynamically impossible; and although to consider the matter from this point of view may help us very much, because it gives us a rough and ready analogy to work on, yet there is great danger that it may stop important advances by an illusive appearance of explanation. In just this way the emissive and elastic solid theories of light and the caloric theory of heat gave apparent explanations of optical and heat phenomena which helped for a long time in advancing these sciences. But these same theories, by being upheld, *after they*

had been conclusively shown to be inadequate, were great stumbling blocks in the way of further advances.

A good deal of importance has been attached, and rightly attached, to the fact, that the heat produced by the neutralization of dilute alkalies by dilute acids is the same as that due to the combination of H and OH. This is certainly a most remarkable fact, but to concentrate attention on it, as if the statement that ions in solution are dissociated took away all the difficulty surrounding the matter, obscures a very important question, namely, why is there then so little heat absorbed when the ions are dissociated by going into solution? It has been proposed to explain this by various suggestions which do little more than re-state the facts in some other form, and call for new properties of ions specially invented to suit the circumstances, which remind one very much of the ingeniously invented properties of light corpuscles that were, one by one, superadded in order to explain optical phenomena.

One way, that depends on a known cause, by which solvents may produce ionization, is by diminishing electrical actions by their high specific inductive capacity. There seems a good deal of reason for this supposition. High specific inductive capacity in a material can hardly mean anything else than that its molecules can have electrical charges easily produced on them at a considerable distance apart. A considerable distance apart, of course, means considerable compared with the distance apart of charges on molecules generally, which seems to be often a very small part (about one hundredth) of the molecular diameter. This follows from considering that the work done in the combination of H and Cl may be mostly due to the attraction of electrons. It is comparatively easy to calculate how far apart at most these electrons can be in order that the known amount of work done in combination may be produced by their approach. When we do so, we find that the electrons must be jammed up quite close together. This is quite accordant with what we know of their independence from spectral observations on molecules; but it at the same time shows how

very much work must be done in order to separate them to even the thickness of a molecule apart. In separating them to a hundred times their former distance, one does 99 per cent. of the work that would be done in separating them altogether. Why is there not an enormous absorption of heat due to solution? The suggestion mentioned is that the presence of a body of high specific inductive capacity, like water, very much diminishes the force of attraction between the electrons, by providing what come to the same thing as induced electrons in the water molecules to help in drawing those in the salt apart. This is an excellent suggestion; but is it not really the very same thing, under another guise, as stating that it is by chemical combination with the water that the salt has conferred upon it the property of exchanging partners? What are these electric charges supposed to be induced on the water molecules, but electrons thereon? and what is the attraction of electrons among molecules but another name for one form of chemical combination? All this hangs together, but it lends no support at all to the dynamically impossible theory that the ions are *free*. What it suggests is that this so-called freedom is due to their being in complete bondage with the solvent. That atoms or molecular groups within a molecule often can and do exchange places is quite in accordance with chemical phenomena. That they should do so of their own accord *when the molecules are arranged in a particular way* is also quite in accord with such phenomena as crystallization, where the molecules, of their own accord, arrange themselves into the crystalline form, *if* they are first polarized by near approach to the surfaces of a crystal, but not otherwise as is evident from the well-known phenomena of supersaturation. These crystalline forces are able not only to arrange the molecules in the solution, but to move massive crystals, and it is an important matter for investigation whether they are simply electrical or of the more complex type of chemical action. This is perhaps the simplest example of so-called catalytic actions where change is induced by the presence of a material which

itself is unchanged, and shows the extensive applicability of the general principle that chemical changes depend upon particular arrangements existing, and go on of their own accord so long as the arranging power exists. It is conceded that electrolysis and its consequences can be explained by this hypothesis; and the only outstanding phenomenon that does not obviously come under this explanation is that of why osmotic pressure is the same for approximately the same number of molecules, and which is supposed to be "explained" by saying that the molecules in the solution are free. This so-called explanation is, however, as I have already pointed out, not a dynamical explanation at all; it is only a very far-fetched dynamical analogy. Thus this supposed advantage of the free ion theory is not only illusory but misleading.*

[* A mode of statement can be formulated which connects all these phenomena, and is in accord with the views expressed by Helmholtz in his Faraday Lecture, as well as the conditions involved in Hess' law of thermoneutrality which are enforced above. It can reasonably be held that it is the transformation of an atom or radicle from electropositive to electronegative or conversely, by interchange of an electron with another atom of different kind, that involves the main thermal change, a mere double decomposition or ionization in which this does not occur, being a slighter phenomenon. Typical examples are, on the one hand,

$$(Na + . Na -) + 2(H + . Cl -) = 2(Na + . Cl -) + (H + . H -),$$

in which one Na − is changed into Na + and in compensation one H + into H −, the thermal accompaniment of the reaction being, in the main, the balance remaining over after these opposite processes; and, on the other hand, a double decomposition such as

$$2(Na + . Cl -) + (H_2 + . SO_4 -) = (Na_2 + . SO_4 -) + 2(H + . Cl -),$$

or an ionization reaction such as

$$(H + . Cl -) = (H +) + (Cl -),$$

arising when, for example, hydrochloric acid gas is absorbed in water, neither of which involves considerable thermal change inasmuch as there is here no interchange of electrons between one kind of atom and another. It is this interchange at the electrodes that is controlled by the electric field in voltaic cells, to drive a definite current; and to that extent the energy involved in this type of reaction is thus rendered mechanically available. The smaller energy set free in reactions in which no electric interchange occurs between the atoms may also be of electric nature; but it cannot be arranged into mechanically available form without a working temperature-fall as in a Carnot engine, except in the case where the solutions are ionized

Helmholtz has called special attention to the superficial electrical layers produced at the contact interface of different materials. He worked at it in connexion with electrical endosmose. The whole subject is replete with interest. We want to know more about this attraction of matter for electricity. To suppose that this is electrical itself is illusory. It would require another force to keep on this permanent electrical charge; and, besides, in our present condition of semi-statical explanation of nature, we must postulate forces that vary according to other laws than the inverse square of the distance in order to produce stability. That there are these superficial actions, which are partly electrical, is beyond doubt. That these electrical layers must produce effects on capillarity is unquestionable. That the forces between bodies in contact are affected by them must be the case. That energy is involved in producing and destroying these electrical layers must be. But how much? is the great question in all these cases. Is the electrical force the most important one acting? Is it the one that pulls hardest? Is it the one upon which most work is done? Is it the one that controls, for instance, the heat produced when insoluble powders are mixed with water? Are the already mentioned superficial crystalline forces that polarize a solution and cause molecules to arrange themselves upon its surface,—are these mainly electrical? Conversely, are the forces that cause solution mainly electrical? The cause of solubility is very imperfectly understood. May we not hope that by following up Helmholtz's investigations of these superficial layers we may discover the causes of solution. The way seems also open for investigating the causes of other catalytic actions, and thus, possibly, of chemical action in general, which may depend essentially on suitable arrangements of molecules. The capture of a comet by the solar system

and the osmotic interdiffusion of ions between different solutions, or between identical ones of different concentrations, can be made of avail. These latter processes, however, take advantage of the energy of simple mixture or dilution, without other change of the ionized solutions.]

depends on a suitable arrangement of the planets. By suitably arranged approaches a set of stars may either stop the relative motion of one of their number so as to cause a resulting binary system, or may confer upon one of their number a velocity such as that of 1831 Groombridge, which, unless it meets with suitable circumstances, will peg away to infinity, as it cannot be stopped by the general attraction of those it is leaving. Two stars coming from a distance will in general recede to the same distance, but two systems of stars need not all do so. After collision there may be re-arrangements of components. Similarly, the approach of two simple systems would, in general, lead to these receding again into the condition from which they came; while the presence of a third system to absorb or re-arrange the energy and moment of momentum of the approaching systems might enable the last state of the systems to differ essentially from that from which they came.

The whole question of the nature and existence of these forces and their connexion with electrical forces, is of the very greatest interest. We may hope that ultimately all forces may be explicable by a kinetic theory of nature; but, so long as no satisfactory theory of the ether exists that explains the very simple electromagnetic laws, we can hardly expect anything more than rough analogies in explanation, or rather illustration, of other causes for these more complex chemic actions. The actions of a medium like a perfect liquid, in intense vortical motion, are capable of illustrating these actions. In the first place, there are possible actions such as those of one vortex on another, which are not propagated from place to place but are due to every vortex, in a sense, occupying all space, and each acting on the other simultaneously everywhere. Each changes the other by a simultaneous action everywhere; there is in such actions no question of propagation. Such actions in a liquid seem to best illustrate gravitation. It is not pretended, of course, that any theory of the nature of this turbulent motion, or of matter, has yet been invented which would lead to the known laws of gravitation. All that is suggested is that in

some action of this kind we may look for an explanation of gravitation, and that in the meanwhile these actions in a liquid are a rough illustration of how actions may exist for which we have no evidence of any propagation. A second class of action of a turbulent liquid is that due to different kinds of polarization of its motion; such, for instance, as that vortex filaments are concentrated in various places, or move through the liquid in various directions. There is every reason for believing that such actions would be propagated from place to place with a velocity depending on the energy per unit volume of the turbulency of the liquid. Such actions, due to polarization of the motion in bulk, can be made to illustrate electromagnetic actions in a wonderfully satisfactory way. Besides these two, there are the possible movements of such things as vortex rings, vortex spheres, and, in general, closed vortices, through the medium. These illustrate for us matter with its molecular structure. But besides these two, there are effects that may be due to the crinkling and waving of individual vortex filaments, to their being distorted from their circular shape of section. It is such action as this that may be expected to illustrate the chemic forces, and to which one would naturally look for an explanation of these actions more complicated than simple electromagnetic action. The question naturally arises, "Can these actions be propagated?" To judge from the analogy, there seems every reason to expect that they could. When we look round for any evidence of propagation of actions other than the already known sound and light vibrations, we find mysterious velocities of propagation of earthquake waves and some chemical actions on sensitive silver salts that have not yet been explained by known material or electro-magnetic laws. These may, of course, be explicable by a combination of known material and electro-magnetic action. Ether waves, of lengths comparable with a millimetre, may act on molecular groups that are much smaller than the length of the wave, in a somewhat similar way to that in which light waves act on the atoms whose diameter is very much less than the lengths of the waves. Vibrations of sound-

frequencies seem capable of helping molecules of iron to set under magnetic force. Long ether waves alter the structure of a metallic powder; may we not expect some in the enormous range between sound and light to disturb chemical equilibrium. Besides the direct propagation of light through matter, there may be electromagnetic actions propagated by the joint action of matter and ether. There is evidence of this in the propagation of cathode rays through solid partitions; and an investigation of this and similar cases will, it is hoped, decide whether these actions are due to the interaction of matter and ether under known electromagnetic laws, or whether we are in presence of a propagation of energy by means of those underlying properties of the ether that seem required to explain fully what we know of chemical actions, and which might be called the chemical properties of the ether.

As we then follow out the directions pointed out by Helmholtz's work, we cannot help being impressed with how far ultimate explanations of nature lead us closer and closer to the conclusion that these phenomena of our consciousness are all explicable as differences of motion. It is the motion which is imposed upon us. Is there not, then, reason in the suggestion that colour and sound, nay, space, time, and substance are functions of our consciousness, produced by it under the action of what may be called an external stimulus, and that the only part of the phenomenon which essentially corresponds to that stimulus is the always pervading motion? And what is the inner aspect of motion? In the only place where we can hope to answer this question, in our brains, thought is the internal aspect of motion. Is it not reasonable to hold, with the great and good Bishop Berkeley, that thought underlies all motion? A purely rational machine might get on very well through the world without believing that other brains than his own had underlying thoughts. It is the position of the consistent positivist. To him nature is what others would call a consistent dream. Such a position posits nothing that is not positively felt. It is consistent, but inhuman. For human life we require

sympathy and affection. For the highest life we require the highest ideal of the Universe to work in. Can any higher exist than that, as language is a motion expressing to others our thoughts, so Nature is a language expressing thoughts, if we learn but to read them? May we not hope that studies of physiological actions, of chemical constitution and change, of vortex motion, of the laws of matter and ether, may some day enable us to discover the motions in our brains underlying sound and light, and smell and touch, and pain and pleasure, hate and love? And may we not hope, then, to be able to form some dim analogies by which we may divine what underlies the much more complex motions of organic nature as a whole, and have a scientific basis for investigating what underlies the whole sequence of organic evolution?

And Helmholtz, by his physiological researches, by his chemical researches, by his physical researches, by his mathematical researches in fluid motion, has advanced mankind by a measurable amount in the road to this splendid goal. By his physiological researches we are measurably nearer a knowledge of the mechanism by which brain motions are affected. By his physical researches we are measurably nearer a knowledge of how to apply the doctrine of the conservation of energy and thermodynamics to discover a dynamical explanation of physical processes. By his chemical researches we are measurably nearer a knowledge of how to apply these same doctrines and electromagnetism to investigate the complex problems of chemical change. By his mathematical researches on fluid motion we are measurably nearer a knowledge of that simplest form of motion in which the postulated properties are so few, that it seems almost the only direction in which we can hope for a really ultimate dynamical explanation of Nature.

And Helmholtz has gone from us. Let us venerate his name.

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THOUGHTS ON THE WORK OF HELMHOLTZ, BEING AN ABRIDGEMENT OF THE HELMHOLTZ MEMORIAL LECTURE

[Hitherto Unpublished.]

HELMHOLTZ was one of the great ones of the nineteenth century; how great we can hardly venture to decide so short a time after his death. His early life does not seem to have given any indication to his teachers or schoolfellows of his wonderful abilities. While forced along lines uncongenial to his growth he made little progress, and the forcing process had fortunately but little effect in contorting his mind into those habits of mere assimilation that are the delight of the ordinary teacher. In the freedom of the German university he rapidly developed. There he learnt how to read nature, and with the freshness of youth grasped at the great principle of conservation of energy then being developed. For the sake of biological students he wrote a paper on the Conservation of Energy which called the attention of the scientific world to the new country they had invaded, and whose splendour the other workers in this field had well nigh overlooked, harassed as they were by the details of their investigations and accustomed as they were to past points of view.

His duties as lecturer in general pathology and physiology at Königsberg led him to feel the want of an instrument for seeing the retina and hence to invent the ophthalmoscope. This raised him at once to such a position by the reputation its great value conferred on its inventor, that he was able henceforth to leave the carrying on of investigations to his students and

assistants, while he devoted his time and energies to devising new investigations and attacking problems whose methods of solution were unknown. How fortunate that he had such assistance: that the endowments of German universities are not frittered away in providing bribes to induce rich fools to improve their intellects by cramming for examinations, an end that could be cheaply attained by smart fines or other inexpensive punishments on those who wickedly neglect the seedtime of youth.

Helmholtz thus obtained time for continuous thought, time to wait for discoveries, time to prepare the ground of thought by familiarity with the problem in all its aspects, time for the evolution of all sorts and kinds of hypotheses, to be rejected one by one, till by a process of natural selection the teeming brain at last saw the true solution. As the natural selection of animals gradually builds up mankind from an undifferentiated jelly, so the conscious selection of mind builds up the great idea from the germs of thought. Continuity and variety are in each case the conditions for rapid progress, and in the brain workshop of a Helmholtz we see the indefatigable workings of a superabundant productiveness bringing forth innumerable varieties for selection. The brain-action is natural too. To Helmholtz the results of his thought are the "natural," nay necessary, outcome of what went before. Is there any essential difference between the productions of nature and of intellectual activity? The actions in the brain of a Helmholtz are to another governed by efficient causes, to Helmholtz they were directed by final causes. Do we not see only efficient causes in nature because we are only observers, not feelers, of the thoughts of Nature?

Of all the children of his intellect Helmholtz seems to have valued most his mathematical investigations, such as those on vortex motion, on discontinuous motion, on hyper-Euclidean space. These seem to have given him most labour to evolve; and in some of them the conclusions seem so simple when pointed out that we are tempted to think our predecessors were

blind not to see the, to us, obvious results, rather than more humbly to credit the discoverer with preternatural insight.

The bearing of Helmholtz's work on chemistry is of most interest to the Chemical Society, and it bears on it in many directions. Lord Kelvin has founded his brilliant suggestion that atoms are vortex rings upon Helmholtz's discovery that vortices in a perfect fluid can neither create nor destroy one another. There are many and serious difficulties in the way of believing in the simplest form of this hypothesis, and the only one that has been at all fully worked out, namely that atoms are thin vortex rings in an otherwise structureless liquid. One of the most serious difficulties in the way of this hypothesis is that the molecules of a hot gas should go more slowly than those in a cool one. This is directly opposed to such fundamental facts as that sound travels faster in a hot gas, and can only be got over by some such improbable suggestions as that when we warm a gas we really deprive it of a great deal more energy than we give it, this abstracted energy being transferred to the ether in a form and by a mechanism that we are as yet unable to control or use. The hypothesis seems also to involve the conclusion that the inertia of matter should change with its temperature. It does not seem quite clear what should then happen to a system going nearly 20 miles a second through space, when we light a candle. Unless some impulse is given to the candle flame, it does not seem quite clear why it is not left behind when its inertia is increased. Very little has been done as regards other forms of vortex motion, and very little indeed in the only direction that seems at all likely to represent nature, where the space between atoms is full of an ether which can be the seat of electromagnetic energy and so must have a complex structure.

Some interesting analogies have been suggested between associations of vortex atoms and chemical combination, but the similarity is so obscured by accompanying wants of likeness that the suggested analogy can hardly be of any use in elucidating chemical phenomena. Of course there can be no

doubt that chemical combination is in all probability a kinetic equilibrium, but we seem very far indeed from being able to dispense with the assistance of gross statical analogies. The tetrahedron theory of carbon has produced singularly valuable results; and the immense simplicity of statical analogies of this kind as compared with any possible kinetic theory makes the statical analogy, for the present at least, a very much more hopeful means of attack than a possibly ultimately more complete kinetic theory. These remarks are intended to comfort chemists who find the vortex atom theories too difficult for useful manipulation, and may wish they were mathematicians to be able to use them. The chemical facts to be fitted by any such theory are as yet anyway simple enough for a mathematician even to understand; and considering the want of success that has attended all attempts hitherto made by most ingenious mathematicians to get a vortex-ring theory that will fit the chemical facts, it seems much more likely to be fruitful for the chemists to work away on statical analogies until the mathematicians have provided some vortex-atom theory that will fit the elementary facts of chemistry with some dim approach to completeness.

Some of Helmholtz's most interesting contributions to chemical physics have been on the theory of electrolysis and on its thermodynamic connexions. He has placed the theory of reversible electrolytic processes on a basis that should give subjects of research for many years to come to students of electrochemistry; and in one direction especially his work has borne most valuable fruit, namely in developing the theory of electric conduction and the nature of solution. By means of semipermeable diaphragms the process of solution can be made reversible and so can be brought within the domain of thermodynamics. Some crude ways of expressing the important analogy between the laws of gaseous pressure and of osmotic pressure have led to differences of opinion as to the legitimacy of *explaining* osmotic pressure by the known dynamical methods of explaining gaseous pressure; but the essential differences and

similarities between the liquid and gaseous states are daily becoming more clearly understood, and there seems every prospect that the whole subject is on the eve of a general consensus of ignorance if not of satisfactory explanation.

How semipermeable membranes act is a very complicated question when we are dealing with those usually employed. They are very complex membranes, and the suggestion that they act as simple sieves is untenable. They are evidently more nearly analogous to Graham's second class of membranes, that act by dissolving some substances and not others, than to his first class which act essentially as mere sieves. This view leads us to hope that we may be able to produce judiciously constructed membranes which like those in animal bodies may be able to act as selective filters, and thus very much increase our methods of chemical synthesis. Hence the search for imperfectly semipermeable membranes seems much more likely to lead to valuable results than the attempt to produce a good semipermeable membrane.

The want of a perfect semipermeable membrane is the less to be regretted in that we can obtain a theoretically perfect semipermeable membrane in the case of solutions of non-volatile salts. The free surface of a liquid is a perfect semipermeable membrane; and it can theoretically be used in such conditions as to prove in a perfectly conclusive form the connexion between lowering of boiling point and osmotic pressure. If a salt solution be placed in the upper part of a tall vessel resting on a perforated plug of some substance that the solution does not wet, the liquid will remain suspended if the perforations be small enough for the capillary surfaces in them to get sufficiently bowed out to support the pressure of the superincumbent column of liquid. It has long been known that the vapour pressure near their bowed-out surfaces is greater than near the flat surface of the same liquid, and we can calculate the increase in vapour pressure in terms of the superficial tension and curvature. Now if pure water be introduced below the perforated plug, evaporation from it and condensation on the

bowed-out surfaces of the solution will go on until the column of solution is long enough to bow out its lower surfaces so far that the vapour pressure near them is the same as that of the pure water. When this takes place, there will be equilibrium; and the pressure of the column of solution will be the osmotic pressure; and the known relation between the vapour pressures at a bowed and flat surface gives the well-known relation between the osmotic pressure and the vapour pressure of the solution. The change of vapour pressure due to substances in solution has been explained as caused by the salt in solution preventing egress, and facilitating ingress of molecules from the gas into the liquid. The well-determined values of this change of vapour pressure show that, at least approximately in the case of dilute solutions, the resulting osmotic pressure is the same as would be produced by the salt molecules in the solution if they could exist as a gas occupying the same volume. Since the lecture was delivered, Professor Poynting has shown that to assume each salt molecule to prevent the egress of a corresponding water molecule without changing the facility for ingress of molecules, would account for this observed value of the osmotic pressure. In the semipermeable membranes usually employed, the conditions are so complicated that it is almost impossible to say exactly what is taking place in them; but the suggestion that has been made that they act like fine capillary tubes, and that the difficulty of entry of the salt is due to an essentially capillary action in these tubes, seems much more probable than that they are mere sieves. In any case the invention of a theoretically perfect semipermeable membrane places the whole theory upon an indisputable basis. The theory that the salt prevents egress of water molecules points much more towards an association of salt with the water than to the complete freedom of salt molecules in the solution. In the case of an electrolyte, there is reason to believe that the two ions of which it is sometimes believed to be composed act independently of one another. They certainly act in some senses independently of one another. The resistance offered by water to the diffusion of a salt may be

calculated from two parts, each representing a resistance to one of its ions. The osmotic pressure may often be calculated on the hypothesis that each ion acts independently of the other. The electrical resistance of a solution can be calculated on the hypothesis that the resistance to motion of each ion is the same as that experienced by them when diffusing. But in view of the enormous rapidity of interchanges, impacts, jostles, etc., that occur in a liquid, it is difficult to say what amount of real independence is hereby postulated.

The theory that the ions are free from one another, and at the same time oppositely charged, is a hardly tenable hypothesis. There must be important actions between these ions and the water in order to prevent their joining together in pairs. The free condition is essentially unstable; and the only hypothesis that has been put forward to account for the very great weakening of the forces of attraction between the ions, which is required in order to account for their independent motion in the liquid, and the small amount of energy required to separate them, namely that they are in presence of water which has a very high specific inductive capacity,—this hypothesis is essentially the same as to suppose that they are in chemical combination with the water. What this suggests is that the so-called freedom of the ions is due to their being in complete bondage with the water.

The theory that the exchanges of atoms which take place in a liquid depend on the advent of favourable configurations is quite in accord with what we know of other chemical actions. The molecules in a supersaturated solution must wait till they are polarized by the presence of a crystal, to arrange themselves; and a similar condition seems to hold in the case of catalytic actions in general. The study of these and other superficial actions, of the electric contact-actions and the consequent electric endosmose, of the heating of powders when mixed with water, and in general of the electric bearings of all these superficial actions, deserves careful attention. Are the forces that cause crystallization electric? Who knows? May we not hope

that by following up Helmholtz's investigations on these superficial layers we may discover the causes of solution, of catalytic actions, possibly of chemical action in general, which may always depend on suitable arrangements of molecules. The capture of a comet by the solar system depends on a suitable arrangement of the planets; may not all chemical actions depend on suitable arrangements of molecules, and the conductivity of solutions upon the opportunities presented by the water for rearrangements of the ions in molecular groups?

It is to be feared that we cannot really explain all these things till we know more of the connexion between matter and the ether, and of the structure of both. We are still in the region of vague analogies. We have studied matter and ether, and are studying phenomena like cathode rays in which we have mixed matter and ether actions, but none of these can be considered as explained till they are all explained on dynamical or other simple principles.

As we follow in Helmholtz's steps we are impressed with how far all explanation consists in a description of underlying motions. The differences we perceive seem to be merely differences of motion. It is the motion that is imposed on us. It is it that we can control or that controls us. Is there not reason then in the suggestion that colour and sound, nay, space, time, and substance are functions of our consciousness, produced by it under the action of what we may call an external stimulus, and that the only part in phenomena which corresponds to the stimulus is what we perceive as motion? And what can be the inner aspect of motion? In the only place where we can hope to answer that question, in our brains, the inner aspect is thought. Is it not reasonable then to hold with the great and good Berkeley, that thought underlies all motion, that thought is the stimulus of our thoughts. A purely rational machine would believe that other brains had no thoughts. To suppose otherwise is unnecessarily to multiply hypothesis. This is the position of the consistent positivist. To him nature is what others would call a consistent dream. Such a position posits

nothing that is not positively felt. It is consistent, but inhuman. For human life we require sympathy and affection. For the highest life we require the highest ideal of the Universe to work in. Can any higher exist than this one; that, as language is a motion expressing to us the thoughts of others, so nature is a language expressing thoughts, if we learn but to read them? May we not hope that the study of physiological action, of chemical constitution and change, of vortex motion, of the laws of matter and ether, may some day enable us to discover the motions in our brains underlying sound, and light, and smell, and touch, and pain, and pleasure, and love? And may we not then hope to be able to form some dim analogies by which we may divine what underlies the much more complex motions of organic nature as a whole, and have a scientific basis for investigating what underlies the whole sequence of human evolution?

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OSTWALD'S ENERGETICS

[From *Nature*, March 12, 1896.]

IN the February number of *Science Progress* there is an interesting article, by Prof. Ostwald, on "Emancipation from Scientific Materialism." There are so many vague fallacies underlying it, that it would hardly be worth answering, only that there is considerable risk that others, chemists especially, may be carried away by the arguments of one whom they rightly value as a leader in their own domain, when he descants positively about the realm of mechanics.

Prof. Ostwald begins by saying that the current view of a mechanical universe fails in two respects, (1) it does not fulfil the purpose for which it was designed, and (2) it is inconsistent with known truths. It is, in the first place, to be remarked that nobody who has considered the matter really seriously can maintain that atoms and motion must constitute the whole universe. Such a view leaves thought out of account, and all that can be held is that material phenomena are so explicable. Prof. Ostwald ignores such theories as that of vortex atoms, which postulate only a continuous liquid in motion; but, it may be, this is omitted because it is merely a way of explaining the atoms. He also ignores metaphysical questions, such as whether motion be not only the objective aspect of thought, and also whether an intuitively necessary explanation of the laws, as distinct from the origin and consequent arrangement of phenomena, is not postulated by the fact that the universe must be intelligible. Consequently his attempt to deal with nature

in a purely inductive spirit is unphilosophical as well as unscientific. The view of science which he puts forward—a sort of well-arranged catalogue of facts without any hypotheses—is worthy of a German who plods by habit and instinct. A Briton wants emotion—something to raise enthusiasm, something with a human interest. He is not content with dry catalogues; he must have a theory of gravitation, a hypothesis of natural selection. This deadly science without hypothesis is far worse than the materialistic *ignorabimus* of du Bois-Reymond; it is the culmination of the pessimism of Schopenhauer.

Prof. Ostwald's first line of attack is that the materialistic hypothesis does not fulfil the purpose for which it was designed. When this is investigated, it turns out that all he means is that everything in nature has not yet been explained on mechanical principles. And long may it be so. The zest of science is discovery. If everything were explained—well, it is so far off we may wait till it comes, to describe what will happen. He notices several things which are certainly not explained yet; such, for instance, as why when atoms combine they produce a result so very different from their components. As nobody has yet suggested any reason why the atoms themselves possess the very curious properties they do, we can hardly expect a satisfactory explanation of why these properties change when they combine. Any way, the existence of an uninvestigated region of this kind does not create any reasonable doubt as to the foundations of the road that has led us well so far.

His second attack is deliberately founded on this, that mechanical hypotheses have not yet been found to explain everything. "I grant," he says, "that for many individual phenomena the mechanical analogues have been given with more or less success. But all attempts to completely represent the whole of the known facts in any department by means of some such mechanical analogue have resulted without exception in some unexplainable contradiction between what really happens and what we should expect from our mechanical model. This contradiction may long remain hidden; but the

history of science teaches us that it sooner or later makes its inevitable appearance, and that all we can say with complete certainty regarding such mechanical similes or analogues—usually termed mechanical theories of the phenomena in question—is that they will doubtless on some occasion fail." All that this really means is that we have not yet explained everything on mechanical principles, and that when we do get a little way on, we are delayed again by something more that requires explanation. But surely this and nothing else is what we ought in all reason to expect. It is about the best test we have that we are on the right track. Prof. Ostwald cites optical theories as an example of the kind of failure he refers to. He seems for some extraordinary reason to imagine that the *elastic solid* theory of the ether is in some curious way specially connected with the mechanical hypothesis of the universe. It is far from being so. The mechanical theory of an elastic solid itself has been only very dimly foreshadowed; and Prof. Ostwald's contention that transverse vibration "presupposes a solid body" is in direct contradiction to Lord Kelvin's theorem that a liquid in turbulent motion could transmit transverse vibrations. Even Lord Kelvin's elastic solid ether in a state of tension could exist if it be infinite, so that here again Prof. Ostwald is mistaken in saying that, because it could not be stable if finite, it can have no physical existence. And finally Prof. Ostwald takes refuge in the as yet unexplained constitution of an ether whose properties were discovered by assuming them to be mechanical, and were only discovered about thirty years ago, and have not been seriously investigated until within the last ten years. Surely no argument can be based upon the fact that there are limits to our present knowledge.

Prof. Ostwald's third attack opens out a new view. We see here a human reason for his desire for emancipation from the mechanical hypothesis. He is dissatisfied with du Bois-Reymond's *ignorabimus*. But even du Bois-Reymond is not infallible, and most prophecies as to the limits of human knowledge have turned out to be only limits to the ingenuity of the

prophet. It is very much more likely that du Bois-Reymond's apparently resistless logic has a flaw, than that the path of progress of science for three hundred years has been along the wrong route. There are plenty of philosophical speculations, which no doubt du Bois-Reymond brushes aside as hardly worth consideration, which would entirely invalidate the greater part of his arguments. Even though they do not, it is certainly quite unscientific to leave a road that has led to great discoveries merely because you imagine that there is some curious spectre in the distance to which you think it is leading you.

Prof. Ostwald's fourth attack is based on the fact that seeds grow into trees, but that trees do not grow back again into seeds. He thinks that if the universe were a mechanical system, there is no more reason for one than the other, and that they should occur equally often. As he says, "the tree could return again to the sapling, etc." But that is not the question. The question is, *must* it, if this is a mechanical universe. The order of events depends entirely, in a mechanical universe, upon the *initial conditions*, and all we can say is that the initial conditions of this earth were such that trees generally grow from seeds, and that the reverse operation has never been known to occur. That it *has* never occurred has nothing on earth to say to the question of whether this is a mechanical universe. As a matter of fact, I believe that this and other much simpler cases, such as are usually classed under irreversible actions in thermodynamics, can be shown to be not only, as I have here argued, *possible* mechanical processes, but to be *the most probable* mechanical processes. Hence it is quite possible that the actual sequence of events which Prof. Ostwald cites as disproving the mechanical theory of the universe may not only be the very best proof extant that the mechanical theory is the most probable theory, but may even lead us to conclude that it is the only possible theory.

Finally, Prof. Ostwald tries to build up something instead of what he thinks he has demolished. A vague energetics is what he presents instead of the mechanics of the past. He advocates

the deadly view that science should be a catalogue, well arranged, no doubt, but free from those dreadful hypotheses. He prefers volume-energy to the molecular theory of gases. He criticises this latter by neglecting to see that the quantity often quoted as energy per cubic centimetre of the gas is really momentum per second carried across a plane, and has consequently that very element of direction which he accuses it of not possessing, and the absence of which in volume energy one might possibly expect him to explain. Prof. Ostwald's idea of science as free from hypothesis is the most advanced form of pure positivism. If he were consistent, he should deny the existence of thought in the moving, coloured, soft objects he sees and feels around him, and calls men. That other men think is a hypothesis, why not this?

In conclusion, Prof. Ostwald seems to have some dim doubt whether energetics will explain everything. As the doctrine of the conservation of energy will not determine by itself the motion of even a single planet round the Sun, it is somewhat curious to see the doubt that seems to haunt him in answering this question. The doctrine of the conservation of energy is most valuable, but it goes only a very little way in explaining phenomena. More than energetics is certainly required unless we are prepared to endow energy with all sorts of curious properties after the manner of our predecessors, who used to invent a new subtle fluid with convenient properties in order to explain every new difficulty. Prof. Ostwald's energy seems more like one of these subtle fluids than any product of modern thought.

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ON SCATTERING OF RÖNTGEN RAYS

[Conjointly with Dr. F. T. Trouton, F.R.S. From *Nature*, April 30, 1896.]

FOR some time past Prof. Fitz Gerald and Mr. Fred. T. Trouton, at Trinity College, Dublin, have sought evidence of crystalline action, both on transmission and reflection at grazing incidence of Röntgen rays. Though so far this has been without success, we learn that they have noticed a marked scattering of the rays, in transmission through some substances. The following arrangement is convenient for showing this:—"On a plate of lead, which has a slit cut in it, is placed a sheet of, say, solid paraffin 2 or 3 mm. thick, so as to cover one end of the slit; over this is laid a strip of lead—but slightly wider than the slit—so as to just entirely cover the slit. No direct radiation then can pass from a Crookes' tube, placed vertically over the slit, to a sensitive plate placed behind the lead; but with a lengthened exposure (20 to 30 minutes) with a focus tube, a darkening is found on developing at the end where the paraffin is placed. If the paraffin be then moved to the centre or other end, so as to eliminate accidental effects, on again exposing the darkening action is found to follow the paraffin. Some darkening always occur even where there is no solid body; how much of this is due to successive reflections from the lead sheet and strip, or how much is due to scattering of the rays by air, is not easy to say."

77

SCIENCE AND INDUSTRY

[Lecture to Irish Industrial League, May 7, 1896.]

I AM sure you are all tired of hearing that science is essential for the success of a people. It is a kind of bugaboo. We are all tired of being frightened by it. If I were not scientifically inclined I know I would hate it, it is so continually being driven down one's throat. This is worse in England than in Ireland, but even here we suffer from it. And yet I will act like the importunate widow, and, though you may hate me, I cry, "Hear me!"

The first thing to understand is the difference between scientific *information* and scientific *methods*. It is, roughly, like the difference between the ingredients of a pudding and the method of making it. A cook might have all the ingredients and yet be totally incapable of making that pudding. Information can generally be easily obtained; but methodical action involves the formation of habits, and habits are of slow growth. Anybody can be told in a few minutes that potato disease is a fungus; but it is very hard to get ill-educated farmers to act as if this were the case, and to refer it to natural causes which they should be in the habit of investigating, and not to some occult influence of the "Season," or of "Thunder," or of "Electricity," or even of "Fairies"—all these being magnificent terms that sound big to them, but have no more meaning than the blessed Mesopotamia that so comforted the poor old lady.

Unless people have scientific habits of thinking and judging,

they cannot distinguish between the value of scientific and unscientific information. They think that muzzling dogs gives them hydrophobia, because they have not formed the habit of distinguishing between *evidence* founded on an orderly and accurate investigation and *prejudice* founded on disorderly and inaccurate hearsay.

All sorts of industries fail because people want scientific information and scientific methods. The knowledge of most farmers about cattle and fowl is disorderly; and they can't improve, because they don't know what suits their case and don't know how to try. They are at the mercy of quacks, and soon cease to believe in the possibility of improvement, and so are beaten by those who can improve.

Take, for example, the case of flax-growing. Flax is grown in Belgium, which, as grown, is much the same as Irish flax, but by its treatment afterwards is worth nearly twice as much. Why can't Irish flax-growers treat it in the same way? Why can't Irish farmers out of Ulster grow flax at all? Partly for want of information, but principally for want of ability to use scientific methods. Scientific methods are: (1) to observe accurately; (2) to experiment accurately; (3) to classify accurately; and (4) to reason accurately. Now, these are the very things in which would-be flax-growers fail. They can't observe accurately when to cut the flax, nor when to steep it, nor when to take it out, nor when to scutch it. They can't experiment accurately, nor verify their observations by experiment to find out what sort of treatment best suits their land, their climate, and their water. They can't classify their observations or their experiments, and consequently they can't deduce any conclusions worth having, nor produce flax worth buying.

Similarly in rearing fowl; similarly in making butter. Why are not all fowl well reared? Why is not all butter good? For want of orderly, accurate treatment—*i. e.* for want of scientific methods.

The failure is largely for want of specific information; and in butter-making, for instance, has been largely remedied by

providing the information. But we cannot be always providing specific information of improved methods every time any industry changes. *Such training is always late.* See how bad the butter-making had to be before this sort of training became possible. It had to be so bad that even the farmers realized that it was bad. The last people to realize that what they produce is bad are the producers. The whole population should be so trained that they are on the look out for improved methods, and should be able to avail themselves of improvements, and even make improvements themselves. We can't be always driving improvements down their throats at the point of the bayonet. They should be willingly sought out and utilized by the producers themselves.

What sort of habits are necessary to be able to do this? Scientific habits. Habits of scientific accuracy in observing, experimenting, classifying, and reasoning. Not theoretical habits, but practical habits. The distinction is most important. Practical habits mean habits of dealing with things. Theoretical habits are habits of dealing with words. These latter are very important and are acquired by dealing with words. Practical habits are equally important for most people, and can only be attained by dealing with things. A clever bookworm once maintained to me that a good experimental education could be got by experimenting, with a dictionary, as to which word fitted a given sentence. He only proved his own ignorance of any practical activity.

We conclude, then, that the way science bears on industry is that, for efficient work, and especially for improvement, for keeping up to the times, for withstanding the competition of the world, for raising themselves above the position of hewers of wood and drawers of water, for preventing extermination like that of the Maori and the Red Indian, a people must be provided with accurate information and with habits of accurate work, *i. e.* with scientific information and scientific methods.

I am afraid the people of Ireland have neither the information or the habits of science. They have had no opportunity of

acquiring either one or the other. The habits are of more permanent importance than the information. Useful scientific information changes from day to day. It can be acquired quickly by those with scientific habits. A gardener who understands the why and the wherefore of what he does, who can do things accurately, who can observe how matters are getting on, who can learn from his experiments and classify his observations, such a one can soon learn to work under new conditions, can apply new information to the special circumstances under which he works, and so can accommodate himself to whatever conditions arise. The gardener who can dig, but because he does not know even *why* to dig, can never know *where* to dig, never rises above the position of a mere digger. The cook who can carry out directions accurately, can observe whether matters are going on as described, can learn from her experimental failures, can classify her observations,—such a one can cook anywhere. The cook who cannot even measure salt accurately, who burns the meat one day and sends it up raw the next, whose information is inaccurate, and her habits inaccurate,—such a one can never be more than a drudge, a failure, a nuisance to herself and to society. Why are the cabdrivers so opposed to mechanically-propelled vehicles? Because they know that they are not able to learn to drive them. They should be the proper drivers. They know the streets. They know how to avoid one another, to turn corners, and so forth; but because they have unscientific habits they don't expect to be employed. It was the same with the handspinners when mechanical spinning was introduced. It is so everywhere. Changed conditions require changed methods; and scientific habits are required to be able to change with the times and avoid extermination.

The cabdrivers are by no means the only persons who should look out for changes now imminent. In London the coachbuilders are all on the *qui vive*. They are learning how to build autocars. They are holding an exhibition of autocars themselves. They don't want to lose the trade because horse

cars are going to be superseded. Surely their energy and enterprise deserve success. No wonder that with such energy and enterprise they get some Irish trade. It would be a most useful work for the Industrial League to press upon the Dublin coachbuilders the desirability of similar activity. Are the heads of the trade in Dublin sufficiently acquainted with scientific principles to be competent to modify their present designs so as to suit autocars? I fear me that the scientific training of many heads of coachbuilding establishments would hardly bear the strain. Why are we so far behind? Why have we not begun building, or at least designing, autocars? Because scientific education, *i.e.* education in practical science, is not very popular in Ireland. Those who expect to be heads of practical business on the Continent learn in Technical Schools and University Laboratories the principles underlying all they do, and so are in a position to direct their works into whatever new channels turn up, and to invent new channels for their energies. The chemical industry was begun in England, and still many departments of it are worked there well; but, I understand, the most advanced departments, the latest improvements, the newest dyes, come from Germany, because German manufacturers believe that by scientific methods they can make improvements and discoveries, and so they employ well-trained chemists to invent improvements and to make discoveries. Would you not be surprised if you heard that the coachbuilders of Dublin had engaged two or three scientific men to experiment and advise upon improvements in coachbuilding, with a view to building autocars? I would be very much surprised, because I don't think Dublin coachbuilders think that scientific methods can improve anything; for they themselves know nothing about scientific methods. And they believe this, though science is thundering at their ears with all the inventions of the last hundred years, by which the social life of civilised man has been revolutionized. I see that silk squirting is to be started somewhere in England. It is a French invention, I believe. Why not have it started here?

Why was it not started years ago? Squirted silk is on the market, and though not so strong as real silk it is a beautiful fabric. We here are dreadfully prone to trust to luck. We think the pneumatic tyres were a lucky stroke, and wish we had the luck to do the same. Was the Bessemer process a lucky accident? Was the Gilchrist improvement a lucky hit? By no means. These were the result of a careful application of scientific principles and of scientific methods of investigation to improve the methods of iron manufacture. We need not expect to be able even to be quick at using such inventions, to say nothing of making them, unless we are able to use scientific methods and appreciate the importance of scientific methods.

See how fast the application of electricity has progressed. Because it was in the hands of those who knew how to apply scientific principles and scientific methods to practical affairs. See how quickly the application of X-rays has progressed in the hands of people trained in scientific principles and in scientific methods. The discovery is hardly five months old, and yet there is talk of its being a necessity in every well-equipped hospital. How old would a discovery in butter-making or flavouring be before it could be introduced into every well-equipped dairy? Why, the method of rotation of crops is more than a hundred years old, and its advantages have been told to three generations of Irish farmers, and yet it has only penetrated a small distance in the backward parts of Ireland. Why? Because, though the farmers were given the information, they were not given scientific habits of thought, and so could not use the information nor appreciate its value.

And so through all industries. Failure is largely due to want of practical scientific information and practical scientific methods, and success due to practical scientific methods and practical scientific information, the methods being in every case the more important for permanent success.

How can people acquire scientific methods? Remember that this means habits of accuracy in observing, in experimenting, in classifying, in judging, and, consequently, in doing. The

only way to acquire habits of doing anything is by doing that thing.

We can't learn to play a violin by reading about it nor even by seeing another person play it. We can't learn to observe things accurately except by observing things accurately. Merely observing accurately the letters that form words in a book will not teach a person to observe accurately events that are changing. Many people who are most accurate about words are very inaccurate and unobservant about things. But it is accuracy in dealing with real things that is necessary for success in industry. The education of our minds and bodies in accurately dealing with things is the great want in our present education. A farmer's boy learns nothing of this kind at school and still less at home. The prospective cook learns very little of this at school and much less at home. During the plastic years of life, while habits are being acquired, the habit of dealing accurately with things is not learned, but, on the contrary, habits of dealing inaccurately are acquired. The boy learns that fairies cause sickness among the sheep, and receives no direction how to seek out by observation and experiment, *i.e.* by scientific methods, for the true natural cause, and so is never able to remove the cause. The girl learns that it's the nature of things to be dirty, and everything "asy" will do "well enough," so she never learns to be accurate or to improve.

The fault of our present system is in supposing that learning to use words teaches to use things. This is at its best. It really does not even teach children to *use* words, it only teaches them to *learn* words, to stuff their memories with phrases, to be a pack of parrots, to suffocate thought with indigestible verbiage. Take the case of experimenting. How can you teach children to make careful experiments with words? Yet it is of great importance that they should be able to learn from experiments. Take the case of spraying potatoes, and look at the reports from all over Ireland published by the Royal Dublin Society. See how many answers run: "I don't think spraying is any good. I spent a lot of trouble on it. I sprayed once,

and my potatoes are no better than those of my neighbours. There was no blight anywhere in my district." How can idiots like that ever succeed? But this is only typical of the way hundreds and thousands of people are arguing in all sorts of matters of vital concern to themselves and others. This sort of reasoning says muzzling dogs is no use and vaccination is a humbug, and as a consequence hydrophobia is rampant, and hundreds of people die per week of smallpox, as in Gloucester the other day. Unless a person is taught young to make proper experiments he won't be able to do it. To suppose that he will, is to suppose that he is a greater genius than Lord Bacon, the founder of experimental investigation. There is no use in expecting everybody to be a genius like this, but almost everybody can be *taught* to make moderately intelligent experiments. But not by words; only by actually making them. And similarly with reference to all kinds of doing, we learn to observe things by observing them, to make things by making them, to classify things by classifying them. The great mistake hitherto made is to suppose that by learning to deal with words children can learn to deal with things; and this mistake is to suppose our poor children are all geniuses like Lord Bacon, and could see how to escape as he did from mediæval methods with words to scientific methods with things.

A somewhat similar mistake has been made in education in the methods of dealing with things. It was for long maintained that *pure* science was the sole business of schools, and that applications of science should be learned in shops and in the field. This, again, assumed that our poor children were geniuses like Watt and Liebig, Lord Kelvin and Sir John Lawes. Geniuses of past generations and of the present are engaged in discovering how to apply pure science to practice. It is no easy matter. People say, "Oh, now, it *only* requires to be applied!" Only! How long was steam known before it was usefully applied? Half the difficulty is in the application, and it requires genius of a high order to apply pure science to practice. And this is what our poor children were expected to do. Science schools

provided them with pure science. They might buy apparatus so long as it had no practical applications. Dynamos are commercial, therefore they are tabooed. Our poor children were, as it were, provided with beautiful fruits that required cooking, but were not allowed to be taught how to cook them. It was known that the method of cooking was the discovery of a few great geniuses, but still the poor children were told, "Oh, you must find out how this is done for yourselves." We are growing out of this stage. Technical schools, where the application of scientific information and methods to practice is taught, are now recognised as necessary for the prosperity of a country. They have long been regarded as necessary in Divinity, Law, and Medicine. Our clergy, our lawyers, and our doctors have for years had technical schools in which to obtain their professional education. Why, then, not our ironworkers, our farmers, our industrial and commercial professions? But technical schools are of little use if proper habits of thought and practice are not produced in our children, for application in the schools. Habits can only be formed young; and one of our principal educational needs now is the need of a department in our schools which will cultivate scientific habits. Other nations have invented ways of doing this. We have only to enter into their labours. Kindergarten has been invented for us in Germany. Sloyd has been perfected in Sweden. Scientific object-lessons and the teaching of elementary science have been worked out in England and Scotland. We here—and we should be ashamed of it—have contributed nothing to the advance of mankind in education. The only remaining direction is *games*. Why should not elementary schools have organized games? They are highly educational. If organized, perhaps they would be kept out of the betting mania. Draughts and chess indoors, hurling and football and cricket out of doors. A practical training in dealing with others, in combining for, and in voluntary obedience to, reasonable rules, in practical initiation, in keeping one's temper, in judging fairly of opponents—these and many other excellent qualities

are produced by school games to an extent that nothing else can emulate. Germany and France envy our school games. Why should not our elementary schools have organized school games.

But this is a digression from Science and Industry.

We conclude that children should be early trained in actually observing, experimenting, classifying, and reasoning from these. This formation of these habits will simultaneously train their hands to do what their heads want, because it is a training in actually doing accurately what their heads want done.

We also conclude that the training should embrace a training in the applications of these habits to the special work of the worker.

This divides the primary from the secondary parts of scientific industrial education.

The business of primary education is the formation of general scientific habits; the business of secondary education is the formation of particular scientific habits.

There is very little chance of creating these habits in the present generation.

They must be spoon-fed with particular improvements like the dairymaids.

This kind of thing can't go on permanently, because it is always too late.

Hence necessity for improved education.

Kindergarten is the beginning of this, but we should have a continuation of kindergarten as the children grow, a growing practical course.

Boys are worse off than girls now—there is needlework for girls.

But we must begin slowly with drawing, both mechanical and freehand.

This is good and useful, but it does not train scientific habits. The children must have opportunities of practically experimenting and classifying and deducing. The old book-worm science that knew much and did nothing is dead.

What subjects lend themselves to this?

Simple physical and chemical experiments and botany. Children can make simple measurements, and the apparatus for experimenting, and can collect and classify—measure up the size of a table, calculate the cubical contents, find its weight, its centre of gravity, find how much salt will be dissolved by water, distinguish between chemical and physical changes—classify plants, slugs, grubs, flies—collect and observe their habits, make experiments on growing plants.

All this takes a long time. We generally expect too much. One or two experiments a month, each conducted carefully and slowly, would be quite enough.

We must carefully recollect that the accuracy cultivated is *not* the accuracy got by repeated practice, but the accuracy of slow carefulness. The accuracy of practice, the rapidity of dexterity, can only be acquired in apprenticeship, in shops, in actual business; but the habit of caution, of accurate verification, of slow carefulness, this can be taught by carefully and slowly doing things accurately and verifying our observations and measurements. A general handiness must also be cultivated and will be cultivated by these methods of education. If not acquired young, the controlling brain-centres become atrophied and incapable of vigorous activity.

Why are we so far behind in all this in Ireland? Is it the fault of the farmers—of the industrial classes? No. It is the fault of our educational system. People with monastic, bookwormy habits of thought have controlled it.

The Intermediate Board won't allow boys in an agricultural country to learn botany. Trinity College won't allow students in their first years to learn experimental science, for fear it might thereby encourage schools to teach children scientific methods. The National Board have not yet made drawing compulsory—the money available a few years ago was divided so as to please teachers and not improve the taught. If all money given be spent on the teachers, they, as a body, must get it all—perhaps not the same teachers as now. But it

should be spent on those who are most useful to the children and their country.

How can we expect any other result when the educational machinery of the country is controlled by a lot of very worthy old bookworms with more sympathy with the theory of equations and Greek verse than with the industrial welfare of the country? How can we expect anything else when Trinity College, that should be a source of light, deliberately tries to prevent schools from training the youth of the country in scientific habits,—and when the Intermediate Board, with ample funds at their disposal, deliberately refuse to take steps to ensure that the knowledge they reward is more than a mere cram of words? The Science and Art Department inspect schools to be sure that practical subjects are taught practically. The Intermediate Board have deliberately refused to do anything of the kind, although it has been pressed upon them. Blind leaders of the blind, we will soon all be in the ditch together.

It is all very well to complain that the industrial classes are not industrious, that they are not cleanly, that they are fond of loafing. This may be all quite true; but who is it sets them the example of being content with what their forefathers did? Who sets them the example of refusing to change with the times? Who sets them the example of behaving like Red Indians and Australian Savages who cannot change with the times, and are consequently being exterminated? It is those who should be their leaders. It is the authorities of the University of Dublin. It is the Board of Intermediate Education. It is the Board of National Education. It is those gentry who think more of sport than of industry, who have left it to the nuns to teach the people to clear away dirt from their houses, and the manure heap from their house-doors.

Awake! O leaders and Press of Ireland, before it be too late—before the people of Ireland are swept away like the Red Indian and the Maori by the competition of a people with capable leaders. The people are starving for bread and you have given them educational stones, and their blood cries out against you.

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LORD KELVIN'S RESEARCHES

[From the *Electrician*, June 19, 1896: in connexion with the celebration of the Jubilee of Lord Kelvin's Professorship at Glasgow. Reprinted with modifications in the Commemorative Volume.]

How different life is at the end of the nineteenth century from what it was at the beginning! Then communication was by stage coach and sailing vessel. The only telegraph a semaphore. All means of distribution of wealth slow and uncertain. Yet what an advance on previous centuries? We can hardly conceive how people lived when hand-spinning and weaving had to provide clothes for everybody. The laws of mechanics discovered in the seventeenth century were rapidly applied in the eighteenth. The experimental discoveries of Boyle and others about gases produced the steam engines of the succeeding years. The seventeenth century may be described as that of the discoveries of dynamics. The eighteenth as that of the discoveries of steam. The nineteenth as that of the discoveries of electricity.

How has all this come about? By patient plodders working on the lines of inductive science. By learning from Nature in the school of experience. All who work on these lines are truly scientific. They may be making experiments on the magnificent scale of laying ocean telegraphs, or on the diminutive scale of measuring the minute change of temperature of melting of ice under pressure; they may be applying mathematics to calculate the oceanic tides, or to determine the size of an atom; but whether they are engaged on the large or the small, on matters immediately applicable to the service of man, or on investigations that are of no more immediately

obvious use than a baby, still they are truly scientific if they learn from Nature in the school of experience. It is the continually learning that is the essence of the truly scientific spirit. Very few have either the ability or opportunity of working on all these lines—of advancing human activity and human knowledge. If one were asked to point out a typical example from those who have applied scientific methods to the advantage of mankind, an example of the kind of intellect that has changed the face of society so that our whole industrial system has been utterly revolutionized and with it the conditions of life of the majority in all civilized nations, the first name that would occur would be that of Lord Kelvin.

He has worked in the most diverse lines. With the whole gamut of applied mathematics at his finger ends he has applied it to the laying of telegraph cables and to calculate how best to signal through them, how best to compensate for the action of the iron in ships on their compasses and how to predict the tides, how to measure temperature and electricity, how to calculate the age of the Earth and the size of an atom. In each of these lines and in many others he has made important contributions to the sum of human knowledge and has improved the opportunities of human activity.

Lord Kelvin was born in Belfast in 1824, and, when only 22 years of age, was appointed in 1846 to the post he still holds. His contributions to the service and information of mankind have been recognized by learned bodies all over the civilized world. An enumeration of the honours conferred upon him by learned bodies would serve no useful purpose here. That he has been so universally honoured confirms our faith that the honour we give him is not an insular prejudice but is founded on true greatness. Even the Crown has been compelled to recognize his valuable services, and has conferred on him first knighthood and then a peerage.

Lord Kelvin's work may be roughly summarized under the following heads:—

(1) Mathematical papers which are mainly concerned with

problems that arose in connexion with special physical questions

(2) Elasticity and optical theory

(3) Hydrodynamics and vortex theory, tides and navigation

(4) Thermodynamics, heat, and the age of the earth

(5) Electromagnetic work, including the laying of cables

Lord Kelvin's mathematics is of the specially powerful type that distinguishes such giants as Laplace and Green. He is determined at all costs to solve the question, and does not give up because it leads to laborious approximations and complicated series. He rejoices no doubt in elegant methods, as, for example, in the theory of electrical images; but what some would consider inelegant and sledgehammer methods are at hand to obtain the result. His mathematics is for the sake of the result, and not for the sake of the mathematics. He has especially developed the methods of physical investigation depending on the principles of the conservation of energy, and the popularity of these methods owes a great deal to his powerful example.

The theory of elastic solids owes a great deal to him. Continental authorities had investigated the question largely from the point of view that solids were collections of points which acted upon one another independently, and, neglecting the pretty obvious fact that their structure and properties could not be explained by any such simple hypothesis, had deduced that the rigidity was necessarily three-fifths of the compressibility in all simple solids. It really came to be a definition of a simple solid; for when it was conclusively shown that plenty of recognised solids had quite other numerical relations between the rigidity and compressibility, all that could be answered was that jelly and cork and indiarubber, and indeed most solids, were not simple solids. Lord Kelvin has always advocated the much more scientific course of first showing inductively from experiment that the knowledge of the properties of elastic solids can be reduced to a knowledge of the two qualities rigidity and compressibility, and then

deducing the behaviour of solid bodies from this. This method is the only satisfactory one open to us in our existing intense ignorance of the structure of solids, and has led in his hands to very valuable results. In Karl Pearson's "History of the Theory of Elasticity" more than a hundred pages are devoted to Lord Kelvin's contributions to the theory of elasticity. The most characteristic peculiarity of these pages is the way in which they bristle with suggestions as to all sorts and kinds of elastic bodies. It is there stated of his work on elasticity: "There is that fertility of idea and that mark of genius which has made Sir William Thomson the leader and characteristic representative of physical science in our own country to-day." The two most generally interesting of his investigations on elasticity are concerned with the elastic solid theory of the ether and with the question as to the rigidity of the Earth. The mere enumeration of these investigations shows how Lord Kelvin has devoted himself to hurling his attacking battalions at the most important and impenetrable of the outworks of our enemy, Ignorance. In each case he has advanced most important trenches against these outworks. In his study of the elastic-solid theory of the ether he has shown, apparently conclusively, that no simple elastic-solid theory is competent to explain the properties of the ether. In his study of the rigidity of the Earth he has shown reasons to believe that the Earth is extremely rigid, reasons which must be taken account of by all future investigators, and which overthrow such suggestions as that the present surface of the Earth is a thin shell floating on a liquid nucleus.

These investigations as to the rigidity of the Earth arose out of investigations as to the tides. From his connexion with submarine cable-laying he has long had an intense interest in navigation, and the theory of the tides and tidal prediction have gained much from him. That the Indian Government have spent large sums of money in providing tide-calculating machines based on Lord Kelvin's inventions is pretty conclusive proof that his investigations in this direction have

borne useful fruit. Without the assistance of these machines the calculation of tide-tables is an extremely laborious process, so much so that it has been seldom undertaken so as to give accurate results in the case of any except the most important ports. With these machines accurate results can be obtained with comparatively little labour.

In other directions Lord Kelvin has assisted navigation. By his inventions, in connexion with the method of sounding by observing the compression of air by the pressure at a depth in the sea, he has notably aided ships in determining their position in thick weather. With the old method of throwing the lead the ship had to be brought almost to a standstill in order to get any result, and this method was entirely inapplicable in deep water unless the ship was quite at rest. With the method introduced by Lord Kelvin the ship may be proceeding rapidly and valuable results obtained in very deep water. The value of the method to all rapid ocean-going steamers is incalculable, and the possibility of safe rapid passages in thick weather is, at least partly, due to the introduction of Lord Kelvin's sounding apparatus on board all large ocean-going steamships. This has been effected notwithstanding the notorious conservatism of sailors, of whom, almost more than of any other class, it may be said:—"People working in established ways have their own work to do and have no time to waste upon 'fads.' So much is this the case that it often happens that they have very little knowledge of how things would work out if they departed from the ruts they are accustomed to run along. That is to say, the spirit of scientific research, which was to some extent present in the industry in its early stages, has nearly all evaporated, leaving behind regular rules and a hatred of fads."

Another important aid to navigation, provided by the spirit of scientific research which certainly never evaporated out of Lord Kelvin, was his compass, with its delicate suspension, slow period, facility for compensation, and freedom from vibration. Though it has been criticised as a scientific toy

and as gimcracky, these criticisms only show the prejudice of the critic, as is proved by the almost universal adoption of these gimcracky scientific toys by those who have had practical experience of them and of others.

Hydrodynamics has benefited in many ways by Lord Kelvin's activity. He has developed important general theorems and applied them to solve many complicated problems. He has studied the waves produced by ships, and advocated bulb keels on a large scale before yacht-builders discovered them. He has largely advanced our knowledge of vortex motion, and has proposed the intensely suggestive vortex-atom theory of matter. All through his work he has been harping on the possibility of explaining elasticity by motion. The stiffness of a gyroscope, the elasticity of a vortex ring, in which movement confers elasticity on inelastic material, have been continually underlying his views of Nature. Our knowledge of how far such things can explain, and how far they have not yet explained, the phenomena of matter and of the ether, is largely due to his initiative. In 1847 he showed how Faraday's ether-forces could be illustrated by the possible displacements of an elastic solid; and though the connexion was a mere analogy, because it made electric and magnetic actions depend upon identical properties of the medium, nevertheless it was the germ from which Maxwell's mathematical representations of Faraday's views may have sprung, and with them all modern developments of electromagnetism.

A generation that has been reared on the law of the conservation of energy, who have imbibed with their earliest scientific milk the equivalence of heat and energy, can hardly understand the position of science when Lord Kelvin began his professoriate. The domain of mathematical physics was then almost entirely in the hands of persons with a taste and training in pure mathematics. The true physical basis of mechanics was passed over as unimportant, and the analysis that solved problems was all important. A generation that called the acceleration of gravity a force, and bewildered the

unfortunate student with it, was by that very fact convicted of carelessness about physical conceptions. A confused nomenclature is an almost certain sign of confused or careless conceptions. Thomson and Tait, by their "Natural Philosophy" and by the influence they have exerted by their teaching, have succeeded in rousing British science from this disastrous position. They called special attention to the law of the conservation of energy and to the physical basis of mechanics at every point. They emphasised the experimental basis of natural philosophy, and took as their text a quotation from Fourier, which clearly points out that our knowledge of the ultimate principles upon which Nature depends is far too imperfect for us to proceed any distance without continual reference to experiment for verification and enlightenment. It is very remarkable how quickly the doctrine of the equivalence of heat and energy spread after Joule's measurements. The old caloric theory had really been disproved absolutely by Rumford and Sir Humphrey Davy in the beginning of the century, and yet it survived until after 1850. It took more than a hundred years to break through the authority of Newton in physical optics, but within ten years after Joule's experiments the equivalence of heat and energy was practically universally acknowledged. In Britain this was certainly largely due to the immense influence exerted by the genius of Lord Kelvin. He seized the great idea, worked out its ramifications into all the corners of physical investigations, and forced it on the scientific world. In a similar way he expounded and amplified the second law of thermodynamics. Even before he saw clearly how to reconcile Carnot's conclusions with Joule's experiments, he extended Carnot's work by founding on it an absolute scale of temperature. He saw clearly that there must be a reconciliation possible, and worked away at the truth on both sides nothing doubting. When the reconciliation was discovered he redoubled his energy and refounded all his former work on the new foundation, while he leaped onwards and propounded the general theorem of

the ultimate degradation of energy, and began his great battle with the uniformitarians of the time who were proclaiming everywhere that there was no evidence in Nature of any beginning or ending of our system. On the one side Lord Kelvin investigated the heat of the Earth and the variations of its temperature downwards, and deduced therefrom a quite moderate antiquity for this globe, while on the other he showed that the Sun himself must die and our whole system perish in night.

The rapid acceptance of the two laws of thermodynamics is unquestionably due largely to Lord Kelvin's genius and energy in extending them to the whole range of thermodynamics, to elasticity, electricity, magnetism, etc., and this is made doubly evident by considering what happened in an isolated district to which his influence hardly extended. Down to the year 1875, in Dublin the acceleration of gravity was called a force, and the second law of thermodynamics was unknown. Only for Lord Kelvin this might almost have been the case all over Britain. When Sir Humphrey Davy was unable to break through the caloric theory notwithstanding his extraordinary reputation, the rapidity with which it was broken by Joule and Thomson is doubly remarkable and is an eternal tribute to their power.

Of all Lord Kelvin's work, that for which he is probably best known is his electromagnetic work. His methods of treating electricity and magnetism have permeated all modern investigations. His instruments are used in innumerable departments of modern research. In two special directions his influence has been specially felt—in the development of the practical measurement of electric and magnetic magnitudes, and in the development of submarine telegraphy.

When Lord Kelvin began working, his intense desire for concrete realisation of everything he dealt with led him to require a numerical measure of electric and magnetic quantities. Weber had shown how this was possible, but still it was not attained by workers in electromagnetics. Each observer

quoted his own galvanometer, or his own battery, or his own copper or iron or platinum wire. Such and such an action "produced 5 scale divisions on my galvanometer." "My electromagnet was wound with 200 turns of fine wire and excited by 20 cells of Callan's battery." Such are the only hints obtainable of what quantities were being dealt with. There was no real attempt to reduce the observations to any common numerical scale. The distinction even between such essentially diverse quantities as current and voltage was very hazy indeed; much the same sort of haziness existed with reference to them as at present puzzles unfortunate students of magnetism in reference to magnetic force and magnetic induction, which are spoken of as if they were quantities of the same kind—an indigestible muddle that the practical electrician has fortunately thrown up, and framed for himself a solid digestible theory of the magnetic circuit. Authorities spoke of quantity-currents and intensity-currents as if they were two utterly distinct kinds of current, one of which produced heat and the other shocks. No accurate ideas were current as to the relations between energy and electromagnetic quantities. All this muddle was intolerable to Lord Kelvin. If there was one thing more than another he objected to, it was to indistinct physical conception and to the consequent impossibility of numerical measure. To him and to others, but in a very large degree to him, is due the international system of electromagnetic measures that exists as one system in all civilized nations. It is almost impossible to over-estimate the value of such a system, coupled as it has been with accurate and easy methods of making the measurements. All who have studied the history of the steam-engine know the rapid strides with which improvement progressed after Watt introduced his method of measuring the power of an engine. The rapid advances and improvements that have been made in the applications of electromagnetic energy are largely due to the great ease of electromagnetic measurements, and this great ease is largely due to Lord Kelvin's influence in providing a scientific

system of units and to his inventive genius in providing accurate and convenient measuring instruments and methods of measurement. One characteristic of his instruments may be worth noting as an evidence of his power, and will be appreciated by all who have experience of scientific-instrument makers. It is that he has been able to impress his own ideas on these makers, and has forced them to depart from their beloved methods of fixing everything as in a vice and of destroying all certainty of adjustment by wrenching everything with gripping screws. To Lord Kelvin is certainly due the merit of introducing rational constraints in scientific-instrument making, and it requires as much determination and ability to force a British workman out of his grooves as to get the Admiralty to accept a new form of compass.

Between 1840 and 1850 telegraphy advanced very rapidly. One expression of those days shows how entirely electric telegraphy has superseded all other methods of communicating by distant signals. Newspaper paragraphs were then headed "By Electric Telegraph." Nobody nowadays talks of *Electric* Telegraphs. There is no need of the "Electric." There are no other telegraphs now. After 1850, submarine telegraphs became of importance and were rapidly extended from the narrower to the wider channels. Attention was early directed to the impossibility of reading signals succeeding one another rapidly through these submarine cables. A good deal of confusion arose as to the nature of this delay in signalling. It was mixed up with the question as to the time taken by the signal to reach the far end of the cable. A mere delay in time would not have been of serious importance; even though a signal took an hour to reach America, it would have been quite unimportant if it had been legible when it did reach its destination. Lord Kelvin quite early in the discussion clearly pointed out the conditions that determined the possible rapidity of signalling. He showed that under ordinary circumstances of signalling the resistance and capacity of the cable were of primary importance, and that self-induction was of a very secondary importance

unless when the cable was coiled up on board ship; and he propounded the law of squares, as it got named, expressing the speed of signalling in cables of different lengths. This question of speed of signalling was of vital importance in respect of long cables, and was hotly discussed in reference to the then being proposed Atlantic cable. In 1855 Lardner writes of this project: "The sanguine consider the project practicable, and its speedy realization probable; the phlegmatic notice it only with ridicule; men of science generally admit the possibility of the enterprise; while men of finance more than doubt the possibility of a remunerative result." As these latter really controlled the situation, and as the remunerative result entirely depended on how many messages could be sent through the cable per day, the whole possibility of the Atlantic cable centred round this question. Mr. Whitehouse, who was intensely enthusiastic in his belief in the success of the enterprise, fought manfully against the law of squares, believing that if this law were true Atlantic telegraphy was practically impossible. He published most interesting results of experiments of cables made with an apparatus in which iron was magnetized by sudden discharges from an induction coil. An interesting revival of this method has been recently employed in Cambridge for studying the still more rapid Hertzian radiations. These results seemed quite inconsistent with the law of squares, and although he was unable to find any flaw in Lord Kelvin's reasoning, he stated that he did not believe in the law of squares, and said that, if it were true, submarine telegraphy across the Atlantic would be impossible. "I can only regard it as a fiction of the schools, a forced and violent adaptation of a principle in physics, good and true under the circumstances, but misapplied here." To this Lord Kelvin replied by reiterating the validity of the law of squares, and by showing that the experiments quoted confirmed the law they appeared at first sight to disprove. In the midst of this controversy the Atlantic cables of 1857 and 1858 were laid. The failure in laying the first, and of the insulation of the second

after being laid, gave rise to immense controversy, but the possibility of laying a cable and of telegraphing through it was conclusively proved. Notwithstanding Mr. Whitehouse's remarks about the law of squares, Lord Kelvin had all along maintained that the law was true and that Atlantic telegraphy was possible. His investigation of the speed of signalling had convinced him that the methods employed were going in the wrong direction. In the 1858 cable large induction coils producing an electromotive pressure of perhaps 2000 volts had been employed, and the failure of the cable had been attributed largely to this excessive pressure. Recent experience of high-pressure transformed oscillating currents would confirm this. Lord Kelvin advocated an entire revolution in all this. He introduced the reflecting galvanometer, with its tiny magnet suspended by a single fibre of silk, and with its beam of light to record its movements. He showed that the conditions for rapid signalling consisted in being able to observe the first beginnings of the current at the far end, and devised the reflecting galvanometer and subsequently the siphon recorder for this purpose. As has been stated by a recognized authority, "the siphon recorder, with its shifting zero, was supremely indifferent to the vagaries of the current which were so destructive of intelligible marks in a dot and dash instrument." As a consequence of these drastic innovations subsequent long cables were worked with electric pressures of a few volts, and the rapidity of signalling enormously increased. Thus it was that long-distance submarine telegraphy became financially successful and consequently possible. Continental authorities on submarine telegraphy agree with British authorities in the belief that Lord Kelvin's instruments have rendered long-distance submarine telegraphy practicable. These instruments, which rapidly revolutionized submarine telegraph practice, were the result of Lord Kelvin's study of the theory of signalling in cables, and are a notable example of an invention founded on abstruse theoretical investigations. Of the actions that take place in cables an acknowledged authority writes: "Some of

these effects have been known and understood from the earliest times, or more correctly since the time when William Thomson taught telegraph cable engineers the principles of their business (in its electrical aspects) and to a great extent the practice too."

Of the living it is hard to write as to their personal characteristics. Everybody who has directly or indirectly been brought into contact with Lord Kelvin has felt the influence of his single-minded enthusiasm for truth, his intense sincerity and sympathetic encouragement. Can a higher tribute be paid to character than to describe it as truthful, sincere, and sympathetic?

Are we not then right in celebrating enthusiastically the jubilee of such a one? He has provided us with noble and magnificent ideas as to the past and as to the future, as to the indefinitely great and the indefinitely small. He has advanced civilization by making the all-pervading ether available for our use, by enabling us to measure its properties, and by teaching us how to lay the nerves of civilization in the depths of the oceans. He has helped to unify humanity, to modify competition by co-operation, to push forward the federation of the world.

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ON THE LONGITUDINAL COMPONENT IN LIGHT

[From the *Philosophical Magazine*, September, 1896.]

In most investigations on the propagation of light, attention has been concentrated on the transverse nature of the vibration. Longitudinal motions have been relegated to the case of pressural waves, and investigators have devoted themselves to separating the two as much as possible. In Sir George Stokes's classical paper on Diffraction, and in Lord Kelvin's Baltimore Lectures, the existence of a longitudinal component is mentioned; but it is mentioned only to show that it is very small and that the motion is mostly transverse. Now the longitudinal component is no doubt generally small, except in the immediate neighbourhood of a source; but it by no means follows that, as a consequence, the actual direction of motion is transverse at all points in a wave. In every complicated wave there are points and often lines along which the transverse component vanishes, and at all these places the small longitudinal component may be, and often is, of great relative importance, so that the actual motion is largely in the direction of wave-propagation at these places.

I. The simplest case is that of a simple oscillator whose theory has been completely worked out by Hertz. There are two kinds of oscillator, an electric and a magnetic one. They are exactly complementary, the magnetic forces in one corresponding exactly with the electric forces in the other.

If the oscillator be taken as an electric one parallel to z, we have for the components of the vector potential

$$F = G = 0, \quad H = H_0 \frac{\cos \overline{pt - qr}}{r};$$

and the components of the electric force, which are in general

$$\dot{P} = \Delta^2 F - \frac{dJ}{dx}, \quad \dot{Q} = \Delta^2 G - \frac{dJ}{dy}, \quad \dot{R} = \Delta^2 H - \frac{dJ}{dz},$$

where

$$J = \frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz},$$

become in this case

$$\dot{P} = -\frac{d^2 H}{dz\,dx}, \quad \dot{Q} = -\frac{d^2 H}{dz\,dy}, \quad \dot{R} = \frac{d^2 H}{dx^2} + \frac{d^2 H}{dy^2}.$$

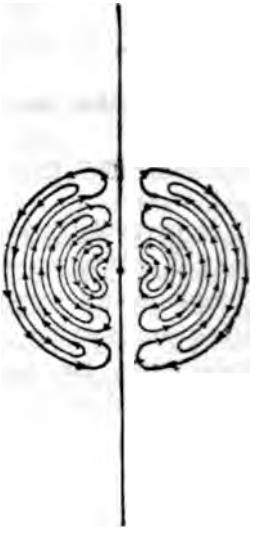
It is particularly to be observed that $\dot{P}$ and $\dot{Q}$ arise entirely from J, which was dismissed by Maxwell as not coming into consideration in cases of wave-propagation on account of there being no varying electrification. This is true as regards *propagation*, but not at all as regards origination. In all cases of origination we have to do with conduction, or its equivalent convection, and in most such cases we have changing electrification which brings in the J term.

The longitudinal component at each point is

$$\dot{\sigma} = \frac{x}{r}\dot{P} + \frac{y}{r}\dot{Q} + \frac{z}{r}\dot{R} = \frac{2z}{r^3}\left(q \sin \overline{pt - qr} - \frac{1}{r}\cos \overline{pt - qr}\right).$$

This is no doubt very small at a distance from the oscillator, compared with the transverse component which involves $\frac{1}{r}$, and in consequence the motion is transverse at most places. On the axis of z, however, the transverse component, which is proportional to ρ the distance from the axis, vanishes entirely. Hence along the axis there is a beam of purely longitudinal vibration, of no doubt small amplitude, but nevertheless existing necessarily in order that there may be no compressions. This all appears on the face of Hertz's investigation. He carefully studied the forces as represented by the above equations, and has plotted them and shown that

they represent a series of whirl-rings thrown off from the oscillator and growing gradually thinner and thinner until at a distance the rings become nearly plane waves, and the opposite sides being always a half wave-length apart are the two opposite phases of the wave. The accompanying diagram roughly represents this state of affairs. It is evident on the most cursory consideration that these waves must have a longitudinal region.* The lines of force in any one wave are up to the axis along any one spherical surface all round; and if there is not to be concentration anywhere, *i.e.* if there is no electrification of the medium, they *must* turn round and be continuous with the return phase of the wave. The reason why they are so feebly concentrated in this return region is because it is so enormously extended. If the wave-length be small compared with the distance from the origin, the flows up and down along the equator are very close to one another and consequently the force is concentrated; while this same force as concentrated within a wave-length has the whole



[* That is, when they are considered as travelling out radially from the vibrator. In a more minute analysis than is contemplated in this paper, the whirl-rings above described are themselves the physical wave-fronts, their motion arising from the surface of each one travelling at each point in the direction normal to itself with the velocity of radiation,—outwards or inwards according to the screw-relation between the directions of the electric and magnetic vectors which both lie along the surface. These vectors are thus at each point both wholly transverse to the direction of propagation. When one of these whirl-rings has been completely formed and broken off from the vibrator, the energy contained in it can be considered as thereafter isolated and travelling out with the ring; for Poynting's theorem gives an energy-flux across the surface, supposed for the moment fixed, which is just sufficient to fill up with energy the space left vacant by the receding ring carrying its own energy along with it. The same considerations regarding the initiation of the radiation are also applicable to more complex types of maintained vibrators.]

hemisphere to return in, and so the longitudinal concentration is quite small, and that is what is represented by the small value of the longitudinal component at any point. The total quantity of longitudinal component must be, on the whole, equal to the transverse component at the equator.

II. In the case of several simple oscillators oriented in different directions the resultant vector potential can be represented by

$$A = U\frac{\cos(pt-qr)}{r} + V\frac{\sin(pt-qr)}{r},$$

where U and V are vectors at right angles to one another. The effect is the same as if two opposite electrons were moving on opposite sides in an elliptic orbit whose plane was that of U and V and whose axes were these two lines.

It is interesting to observe that this case, coupled with a slow rotation of the ellipse which would be produced by almost any small disturbing force in its plane, has been shown by Dr. Stoney to be a sufficient cause for the double lines in spectra which are so common and which are familiar to everyone in the double sodium line.

If the directions of U and V be taken as those of x and y, and z be taken perpendicular to the plane of this ellipse, we may take

$$F = F_0\frac{\cos(pt-qr)}{r}, \quad G = G_0\frac{\sin(pt-qr)}{r}, \quad H = 0,$$

and we get a sort of corkscrew wave with a longitudinal component which can be represented by

$$\sigma = \frac{\sin\phi}{r^2}\{L\cos(pt-qr+l)\},$$

where ϕ is the angle between r and z, and L and l are functions of F_0, G_0, r, θ, and q.

This component vanishes along the axis perpendicular to

the plane of the ellipse, and is a maximum in this plane. If $F_0 = G_0$, this simplifies to

$$\dot{\sigma} = \frac{\sin\phi}{r^2}\left\{2q\sin(pt-qr-\theta)+\frac{1}{r}\cos(pt-qr-\theta)\right\}.$$

This case is rather interesting, as being the form of magnetic wave that is thrown off into space by the rotation of each of the Earth's magnetic poles.

The more complex wave thrown out by the Earth with its two magnetic poles comes under the next head; but it is waves of this type which must be thrown off by the planets rotating round the Sun, if they are electrified, and by their gravitating property if gravitation be propagated in the same way as electromagnetic disturbances.

III. We can produce any desired combination of complex doublets by operating on a simple doublet with a function of $\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}\right)$. The typical term of such a function may be taken as

$$\left(\frac{d}{dx}\right)^\alpha\left(\frac{d}{dy}\right)^\beta\left(\frac{d}{dz}\right)^\gamma = \delta.$$

If we write

$$u = \frac{\cos(pt-qr)}{r},$$

we get as a typical case,

$$F = \delta u, \quad G = H = 0.$$

Also, remembering that $\Delta^2 u + q^2 u = 0$, we have for the electric force corresponding to this typical case of a vector potential,

$$\dot{P} = -q^2\delta u - \delta\frac{d^2u}{dx^2}, \quad \dot{Q} = -\delta\frac{d^2u}{dx\,dy}, \quad \dot{R} = -\delta\frac{d^2u}{dx\,dz}.$$

Now this operation will introduce all sorts of powers of $\frac{1}{r}$ and of q, and I only want to calculate the principal term in the longitudinal component. In making this approximation we may simplify the calculation by observing that the largest

terms are always due to differentiations with respect to the circular part of u, and that differentiation with respect to x, y, z or r lowers a term by one. We may then leave out all differentiation with respect to coordinates outside the circular part in terms of the second order, and it is well to reduce the differentiations represented by δ so as to produce terms of the form $\delta \frac{\cos(pt-qr)}{r}$ and $\delta \frac{\sin(pt-qr)}{r}$. Of course it very much simplifies calculation to use the typical form e^{-iqr} for the circular functions.

We thus get for the values of the components of electric force to the second order* :—

$$\dot{P} = -q^2 \frac{y^2+z^2}{r^2} \delta \frac{\cos \overline{pt-qr}}{r}$$

$$-\frac{q}{r}\left\{1+2\alpha-2\frac{x^2}{r^2}(\alpha+\beta+\gamma+\tfrac{3}{2})\right\}\delta\frac{\sin\overline{pt-qr}}{r},$$

$$\dot{Q} = q^2 \frac{xy}{r^2} \quad \frac{\cos\overline{pt-qr}}{r}$$

$$-\frac{q}{r}\left[\alpha\frac{y}{x}+\beta\frac{x}{y}-2\frac{xy}{r^2}(\alpha+\beta+\gamma+\tfrac{3}{2})\right]\delta\frac{\sin\overline{pt-qr}}{r},$$

$$\dot{R} = q^2 \frac{xz}{r^2} \delta \frac{\cos\overline{pt-qr}}{r}$$

$$-\frac{q}{r}\left[\alpha\frac{z}{x}+\gamma\frac{x}{z}-2\frac{xz}{r^2}(\alpha+\beta+\gamma+\tfrac{3}{2})\right]\delta\frac{\sin\overline{pt-qr}}{r}.$$

In this form it is evident at once that the highest terms vanish in the longitudinal component

$$\dot{\sigma} = \dot{P}\frac{x}{r}+\dot{Q}\frac{y}{r}+\dot{R}\frac{z}{r}$$

$$= -\frac{q}{xr^2}\{\alpha(y^2+z^2)-(\beta+\gamma+2)x^2\}\delta\frac{\sin\overline{pt-qr}}{r}.$$

[* The values of $\dot{P}$, $\dot{Q}$, $\dot{R}$, $\dot{\sigma}$ are here printed as re-calculated by Prof. Thrift. The quartic cones mentioned on p. 424, over which $\dot{\sigma}$ vanishes, should therefore be simple quadric cones.]

In order to get this we have to observe that, when applied to the circular part only,

$$\delta = \left(\frac{qx}{r}\right)^{\alpha}\left(\frac{qy}{r}\right)^{\beta}\left(\frac{qz}{r}\right)^{\gamma}.$$

Any particular typical term of this order vanishes over the quartic cone given by the coefficient of $\delta \frac{\sin \overline{pt - qr}}{r}$.

This is the cone of intersections of the systems of spheres

$$x^2 + y^2 + z^2 = 2\theta x$$

with the cubics

$$\theta\{x^2(1-\beta-\gamma) + a(y^2+z^2)\} = ax^2(y+z) + y^2\{(a+\beta+2\gamma-\tfrac{3}{2})x + \beta y\} + z^2\{(a+2\beta+\gamma-\tfrac{3}{2})y + \gamma z\}.$$

In the particular case of a series of end-on vibrators for which $\beta = \gamma = 0$, this cubic breaks up into the plane $x = 0$ and the quadric cone

$$y^2 + z^2 = \frac{a}{a(\theta - 1) + \frac{3}{2}} x(y+z).$$

In every case the quartic cone intersects every plane perpendicular to the axis of x in a bicircular quartic.

In the case of a complex oscillator whose components are not all parallel to the axis of x, as in the case just studied, the longitudinal component will vanish to this order over a quartic cone so long as we confine ourselves to a typical term δ. This cone is of the form

$$U = (x^2+y^2+z^2)(a_1x^2 + b_1y^2 + c_1z^2) - \{ax^3(y+z) + by^3(z+x) + cz^3(x+y) + ly^2z^2 + mz^2x^2 + nx^2y^2 + xyz(px+qy+rz)\}.$$

In general it is quite evident that the motion along the radius does not vanish.

On considering the general case, we may observe that if the differentiations involved in δ are such that for every term $\alpha + \beta + \gamma$ is either even or odd, then there will be a complex

surface all over which the normal component will vanish to the second order of small quantities;* but that if $\alpha + \beta + \gamma$ be even in some terms and odd in others, we shall have $\dot{\sigma}$ of the form

$$\dot{\sigma} = U \cos \overline{pt - qr} + V \sin \overline{pt - qr},$$

and this will only vanish to this order over the curve of intersection of $U = 0$ and $V = 0$.

IV. If we now consider the case of diffraction through a narrow aperture, it is simpler to take the case of the electric displacement of the incident wave as parallel to the edges. In this case the electric force is everywhere parallel to the edge, and consequently its longitudinal component everywhere vanishes. On the other hand the magnetic force is perpendicular to the slit, and has a longitudinal component everywhere except in the plane through the slit perpendicular to the wave-face. In considering the more complicated case of the electric displacement being perpendicular to the slit, it is necessary to take account of the nature of the edges, whether they are non-conductors or conductors, whether they are crystalline, and so forth, because their electrification, etc., must come into consideration. Similarly, in the case of the electric displacement being parallel to the slit, the magnetic properties of the edges may be important. In this case, too, their conductivity influences the effective width of the slit, as is evidently the case when we are dealing with wire gratings in the path of the Hertzian radiations. These questions are involved in a complicated way in the whole discussion of the effect of a grating on the plane of polarization of the incident light. I will take the simple case of a slit bounded by obstacles which completely stop all action. Although such do not exist, very close approximations to them do exist.

If we take the slit as parallel to z, and make this axis the centre of the slit, and assume the phase the same all over the

[* *i. e.* to the second higher power of r^{-1}, as compared with the transverse component.]

slit, we have for the vector potential at a point x_0, y_0, 0, due to any line of the slit at a distance y from its centre,

$$h = 2H_0 \int_0^\infty \frac{\cos \overline{pt - qr}}{r} dz,$$

where

$$r^2 = x_0^2 + (y_0 - y)^2 + z^2.$$

Integrating this for the width of the slit, *i.e.* from $+b$ to $-b$, we get for the complete value of the vector potential

$$H = 2H_0 \int_{-b}^{+b} \int_0^\infty \frac{\cos \overline{pt - qr}}{r} dz\, dy.$$

When we are dealing with the case of b being a small quantity we may take

$$\int \frac{\cos \overline{pt - qr}}{r} dy = f(y_0 - y),$$

and we have

$$\int_{-b}^{+b} \frac{\cos \overline{pt - qr}}{r} dy = f(y_0 + b) - f(y_0 - b)$$

$$= 2b\left(\frac{df}{dy}\right)_0 + \frac{2b^3}{1.2.3}\left(\frac{d^3f}{dy^3}\right)_0 + \ldots$$

But

$$\left(\frac{df}{dy}\right)_0 = \frac{\cos(pt - qr)}{r} = u,$$

and when $y = 0$ is put in,

$$r^2 = x_0^2 + y_0^2 + z^2 = \rho^2 + z^2,$$

therefore

$$\int_{-b}^{+b} u dy = 2\left(bu + \frac{b^3}{1.2.3}\frac{d^2u}{dy^2} + \ldots\right).$$

If we now integrate with respect to z we get

$$H = 2H_0 \int_0^\infty \int_{-b}^{+b} u\, dy\, dz = 4H_0\left(b\int_0^\infty u\, dz + \frac{b^3}{1.2.3}\frac{d^2}{dy^2}\int_0^\infty u dz + \ldots\right).$$

Now $\int_0^\infty u dz$ is a function of ρ only, and is a Bessel function

subject to the equation

$$\frac{d^2J}{d\rho^2} + \frac{1}{\rho}\frac{dJ}{d\rho} + q^2J = 0,$$

so that we can write

$$H = 4H_0\left(J + \frac{b^2}{1.2.3}\frac{d^2J}{dy^2} + \dots\right)$$

$$= 4H_0\left[J + \frac{b^2}{1.2.3}\left\{\frac{1}{\rho}\frac{dJ}{d\rho} + \frac{y^2}{\rho^2}\left(\frac{d^2J}{d\rho^2} - \frac{1}{\rho}\frac{dJ}{d\rho}\right)\right\} + \dots\right].$$

By means of the differential equation we may, of course, express all the differentials of J in terms of J and $\frac{1}{\rho}\frac{dJ}{d\rho}$. We may, however, simplify matters very much in the ordinary case of light by observing that q is generally a very large number, so that terms involving its powers are large. Keeping to these we see that $\frac{d^2J}{d\rho^2} = -q^2J$, and that the highest term in $\frac{d^nJ}{d\rho^n}$ is $\left(\frac{qy}{\rho}\right)^n J$. Using these terms only, we get

$$H = 4H_0J\left(b - \frac{b^3}{1.2.3}\left(\frac{qy}{\rho}\right)^2 + \dots\right)$$

$$= 4H_0J\,\frac{\sin\frac{bqy}{\rho}}{\frac{qy}{\rho}} = 4H_0Jb\,\frac{\sin\epsilon}{\epsilon}, \text{ if } \epsilon = \frac{bqy}{\rho}.$$

Without going into the question as to the best series to express J by, it is evident from its integral form and from the dynamics from which it is derived that it must represent a wave-propagation. In fact by integrating by parts it could be expanded in the form

$$J = J_1\cos(pt - q\rho) + J_2\sin(pt - q\rho).$$

In any case we can see that for any constant value of ρ, H passes through a series of values giving the alternate lights and darks on a screen illuminated by a narrow slit.

Considering now the magnetic force, we have

$$\alpha = \frac{d\mathrm{H}}{dy}, \quad \beta = -\frac{d\mathrm{H}}{dx}, \quad \gamma = 0,$$

and hence the longitudinal magnetic component

$$m = \frac{d\mathrm{H}}{dy}\frac{x}{\rho} - \frac{d\mathrm{H}}{dx}\frac{y}{\rho}.$$

From this it is evident that in every such case $m = 0$ so far as H is a function of ρ only. Thus we get

$$m = 4\mathrm{H}_0\mathrm{J}\frac{b^2q}{\rho}\frac{x}{\rho}\frac{\epsilon\cos\epsilon - \sin\epsilon}{\epsilon^2}.$$

This shows that m does not, in general, vanish, but has alternations of value like H. The tangential component has for its most important term

$$\tau = 4\mathrm{H}_0\frac{d\mathrm{J}}{d\rho}\frac{\sin\epsilon}{\epsilon}.$$

It is evident that this longitudinal displacement is necessary at the edge of the beam in order to prevent any concentration of the magnetic force. So far as our *a priori* knowledge of pure ether is concerned, there seems no sufficient reason for not supposing a concentration of magnetic force just as probable as one of electric force. It would certainly complicate our equations very much to suppose both. If both existed we might have two kinds of pressural waves, one a wave of electric condensation and rarefaction, and the other a wave of magnetic condensation and rarefaction.

It is quite evident from all these cases and from general considerations that the edge of every beam of light is bordered by a region where there are longitudinal vibrations taking place.

V. As a final example I take the case of a series of slits forming an optical grating.

In this case the simplest supposition is to assume that the opacity of the grating varies in a simply periodic manner. This leads to the same sort of equation for H as in the last case, except that the intensity in each line is proportional to $(1 + \cos ly)$, where $l = \frac{2\pi}{s}$ and s is the interval between the lines.

This leads to the integral

$$H = 2H_0 \int_0^\infty \int_0^\infty \frac{(1 + \cos ly) \cos (pt - qr)}{r} \, dy \, dz,$$

where $$r^2 = x_0^2 + (y_0 - y)^2 + z^2.$$

Now, from general considerations, it is evident that it must be possible to expand this in terms of $\cos ly$ by Fourier's theorem, so that

$$H = h_0 + h_1 \cos ly + h_2 \cos 2ly + h_3 \cos 3ly + \ldots$$

Observing then that H being a function of x_0 and y_0 only satisfies the equation

$$\frac{d^2H}{dx_0^2} + \frac{d^2H}{dy_0^2} + q^2H = 0,$$

we get that in general

$$\frac{d^2h_n}{dx_0^2} + (q^2 - n^2l^2)h_n = 0,$$

so that

$$h_n = H_n \cos \sqrt{q^2 - n^2l^2}\, x,$$

so long as nl is $< q$; and when nl is $> q$

$$h_n = H_n e^{-\sqrt{n^2l^2 - q^2}\, z},$$

as the value cannot increase to infinity.*

[* *i. e.* as *infra*, no radiation of wave-length greater than the interval between the lines can penetrate across any uniform grating so as to form a spectrum. At a grating composed of conducting wires, the component of electric radiation which has its alternating electric force directed along the wires is *in all cases* wholly stopped, because the impossibility of the existence of finite electric force in the wires themselves prevents such a purely longitudinal field of force from penetrating into the spaces between them.]

We thus get the general form for H,

$$H = H_0 \cos(pt - qx) + H_1 \cos ly \cos(pt - \sqrt{q^2 - l^2}x) + \dots$$

$$+ H_n \cos nly \cos(pt - \sqrt{q^2 - n^2 l^2}x) + \dots$$

$$+ H_m \cos mly \, e^{-x\sqrt{m^2 l^2 - q^2}} \cos pt + \dots$$

It would appear from this that at the surface of the grating, where $x = 0$ when $t = 0$,

$$H = H_0 + H_1 \cos ly + \dots + H_n \cos nly + \dots$$

It would consequently seem that this must, in general, represent the distribution of opacity at the grating, and that in the case of a simply periodic distribution the general form of H would be

$$H = H_0 \cos(pt - qx) + H_1 \cos ly \cos(pt - \sqrt{q^2 - l^2}x).$$

We thus get an interesting form for the double integral for H.

The magnetic force to be calculated from this is

$$\alpha = \frac{dH}{dy}, \quad \beta = -\frac{dH}{dx}, \quad \gamma = 0,$$

and consequently

$$\alpha = -lH_1 \sin ly \cos(pt - \sqrt{q^2 - l^2}x),$$

$$\beta = qH_0 \sin(pt - qx) + \sqrt{q^2 - l^2}H_1 \cos ly \sin(pt - \sqrt{q^2 - l^2}x).$$

In this, α is the longitudinal component of the magnetic force. This represents a series of waves being propagated away from the grating, together with a series of elliptic whirls whose length is $\frac{2\pi}{l}$ and breadth $\frac{2\pi}{\sqrt{q^2 - l^2}}$. The length is the same as the width of the lines of the grating, and the breadth somewhat greater than the length of a wave $= \frac{2\pi}{q}$. It is especially obvious in this case that some longitudinal component exists.

The existence of the terms depending on $e^{-\sqrt{m^2l^2-q^2}x}$ shows that there may be something analogous to total reflexion with its extinction wave in the case of a grating in respect of the spectra that are of a higher order than can be transmitted by the grating. It would seem, then, that the whole energy of the wave might not be distributed over the spectra unless the variation of opacity in each line be judiciously made. This may also be connected with the high absorbing and radiating powers of rough surfaces and with the action of coherers.

It is a matter for consideration whether it would not be worth while manufacturing photographic gratings by causing the two first spectra on each side of the central image, together with this central image, or without it, to interfere on the surface of a sensitive film. We might thereby produce a grating which had such a distribution of opacity as to reproduce only these first order spectra and have all the light that passed through concentrated in them. Similarly we might manufacture a grating which would have the light concentrated in any desired pair of spectra, though this would practically come to the same thing as the first proposal, with the lines closer together. This comes to the same thing as producing gratings by means of the interference of two beams of parallel rays of monochromatic light in the manner that Wiener has shown to be possible.

In all these cases it is quite evident that a longitudinal component of either electric or magnetic force is essential to the existence of waves whose intensity is not constant all over their surface, and that it is a practically universal concomitant of all waves of noncondensational type. In the case of short waves which vary slowly from point to point, the intensity of the longitudinal component at any place will be in general very small, because the area is very large over which the motion along the surface at one place has at its disposal in which to turn and be continuous with the motion back along the face of the next wave. This does not make it unimportant, however. In a great many cases the total flow along the face of

a wave must somewhere flow longitudinally so as to be continuous with the flow back along the other face of the wave. Unless these longitudinal flows are taken into consideration the whole energy of the wave is not accounted for. If the rate of variation of intensity over the surface be comparable with a wave-length, as in the case of fine gratings, the longitudinal component is a large part of the phenomenon, and, in fact, represents a large part of the energy in this case transmitted to the secondary image. This is all quite obvious in the case of gratings from the ordinary theory, for the equations given as a solution of this case represent a series of waves being transmitted in different directions from the grating corresponding to the directions of the secondary spectra.

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HERTZ'S MISCELLANEOUS PAPERS*

[From *Nature*, November 5, 1896.]

ANYTHING written by Hertz is of interest; and these papers are of interest, not only on this account, but also on account of their suggestiveness. It is always a question as to the desirability of republishing and translating papers published some years ago. Most valuable papers of ten years' standing have produced their effect. Their vitality has been transmitted to and reproduced in subsequent work; but what the scientific world requires is advance rather than revision. The work of pioneers is, however, largely an exception to this rule. They are generally in advance of their times, and much of their work is of value long after it was done. Such an one was Hertz. Most of his papers are suggestive of questions which still require answers; and they all breathe a spirit that, as he says himself of Helmholtz's work, evokes "the same elevation and wonder as in beholding a pure work of art." His papers are not mere enumerations of observations, nor mathematical gymnastics. Each has a definite purpose and an artistic unity. A life-giving idea pervades it. It is no mere dry bones, but an organic whole that lives for a purpose, and does some work for science.

Prof. Lenard has earned much gratitude for his Introduction. It gives a charming picture of Hertz, of his simplicity, his devotion to science, his loving regard for his parents.

* "Miscellaneous Papers." By Heinrich Hertz. With an Introduction by Prof. Philipp Lenard. Translated by D. E. Jones and G. A. Schott. Pp. 364. (London and New York: Macmillan and Co., Ltd. 1896).

There is just enough added to the very well selected letters to give the reader a continuous view of Hertz's work, and enable him to follow its development, and hence feel an interest in it and sympathy with the worker, thus fulfilling the best ideal of the biographer.

One of Hertz's first investigations was as to the kinetic energy of an electric current. The question is still of great interest. It is known that the magnetic induction that accompanies an electric current behaves exactly as if it were a mass moving with inertia. This is the inertia of magnetic induction. Hertz was, however, looking for a different inertia. He looked at the subject from the flow of electricity point of view. He thought that there might be some phenomenon corresponding to an inertia of the electric charges, which upon this theory are supposed to be flowing in opposite directions through a conductor. He supposed that these might have some inertia *in addition* to the magnetic inertia which accompanied their motion. To test this he tried two different forms of experiment, and obtained results which showed that if there were inertia of this kind, it must be small compared with that of the magnetic kind. The first method consisted essentially in a careful comparison of the extra current in a conductor with its calculated value; the second consisted in observing whether anything like the action by which the trade winds are deflected from a due northerly and southerly flow by the rotation of the Earth, could be observed in a rotating conductor when traversed by an electric current. That there is some directed inertia in the conductor when traversed by an electric current is very probable, and in some cases we can be sure it exists. Hertz himself remarks that the inertia of the motion of the ions in electrolysis is considerably greater than what he was looking for in a metallic conductor. He could not make sufficiently delicate experiments with his apparatus to detect it, however, when using the small densities of current that were available in liquids; but the question is of great interest, and deserves further investigation. There can

be no doubt that in gaseous discharges, kathode rays, as well as in electrolysis of liquids, there is a directed motion of matter accompanying the electric current which would be of the nature of the inertia Hertz was looking for, but failed to find. There seems much reason for thinking that in metallic conductors some similar actions are also taking place. Besides all this, there is the question as to how far the theory that all electricity is molecular and consists of electrons, involves the supposition of an inertia of this kind. Is the inertia of an electron completely specified by the magnetic force accompanying it? Does it occupy no space itself, and is its external field its all? We are hardly in a position to answer such questions. We might, however, be able to answer the former question, as to the inertia of the directed matter movements accompanying the current,—and as to another interesting question of a similar kind, namely, as to how far we can legitimately assume the current inside a conductor to be absolutely homogeneous. The self-induction of a single wire of a square mm. in section is not exactly the same as that of, say, a hundred wires each of the thousandth of a square mm. in section, and distributed over the square mm. Subdividing the current would increase its self-induction. Outside the wire the distribution of magnetic force would be practically the same as before; but inside we would have it concentrated into a hundred small wires instead of being uniformly distributed, and the effect of this would be to slightly increase the self-induction, and the more so the smaller the section of each wire into which the square mm. were subdivided. Hence we conclude that if the current in a real wire be from molecule to molecule, and so be concentrated on certain lines, its inertia should be somewhat greater than that calculated from the hypothesis that it is uniformly distributed over the section of the conductor. The difference between these two views is most clear when we consider the case of a Leyden discharging by its insulating medium becoming a conductor. If the Leyden be completely closed, and the medium become

a conductor in such a way that the strain in each cubic cm. is there destroyed by conductivity, there will be no magnetic force anywhere accompanying this discharge of the Leyden, and consequently no magnetic inertia, if the conduction be perfectly homogeneous. Now it seems almost impossible that any directed change can take place without some accompanying inertia, and we may consequently conclude that either (*a*) an electric current has inertia such as Hertz was looking for, or (*b*) electric conduction currents are essentially heterogeneous, or (*c*) electric conduction is essentially accompanied by material inertia, or (*d*) two or all three of these are true. That (*c*) certainly exists in this case is incontestable in view of the known directed strains that Kerr and Duter have proved to exist in matter subject to electric stress. What is the complete answer, is the important question. It is still unsolved. It lies at the foundation of every theory of electric conduction. Hertz attacked it. It is still waiting solution.

The papers on the contact of elastic spheres and on hardness are most valuable contributions to the subject. They place the question of hardness on a scientific basis, and lay the foundation for a quantitative study of this most variable property of matter. There is no quality in which different materials differ more than in hardness. Electric conductivity is perhaps as various as hardness, compressibility, and viscosity, but hardly any other quality of matter is at all comparable with these in variety of range. Of these hardness is one of the most important and least known, and since Hertz's work on it can be scientifically studied. Innumerable subsidiary questions arise in connexion with it. Why are some bodies so easily polished? Is the polishing of marble connected with the ease with which crystals of calcspar can be twinned by pushing over one corner? What is the essential difference between polishing and grinding? What is the effect of impurities on hardness? Is it comparable to their effect on electric conductivity? What is the cause of this effect?

In considering the cracking of a material like glass, Hertz

seems to think that its cracking will depend only on the tension; that it will crack where the tension exceeds a certain limit. He does not seem to consider whether it might not crack by shearing with hardly any tension. It is doubtful whether a material in which there were sufficient general compression to prevent any tensions at all, would crack. Rocks seem capable of being bent about and distorted to almost any extent without cracking, and this might very well be expected if they were at a sufficient depth under other rocks to prevent their parts being under tension. It is an interesting question whether a piece of glass could be bent without breaking if it were strained at the bottom of a sufficiently deep ocean. On the other hand, there seems very little doubt that the parts of a body might slide past one another under the action of a shear, and would certainly crack unless there were a sufficiently great compressive stress to prevent the crack; and that consequently a body might crack, even though the tensions were not by themselves sufficiently great to cause separation, and might crack where the shear was greatest, and not where the tensions were greatest.

Then follow some papers on hygrometry and evaporation. A very interesting point is raised in this latter connexion. Can a liquid evaporate at an unlimited rate if the vapour produced is removed as rapidly as it is evolved? From two points of view Hertz shows that there is a limit, and by his experiments went far to show that there was no other cause limiting the rate of evaporation. The first point of view was that a limit is imposed by the difficulty of supplying heat sufficiently rapidly to keep the surface temperature constant. He does not seem in his experiments to have attempted to supply this by radiation, but was content to allow the liquid to supply itself by conduction and convection from below. The second point of view was that the molecules could not leave the surface faster than they would be moving in the vapour that was formed. Hertz's investigation of this case only assumes an average velocity for the molecules; he does

not consider the distribution of velocity among the molecules, nor whether they escape equally easily in all directions. The experimental investigation of the conditions of evaporation is extremely difficult; and until some more satisfactory method of studying these conditions be invented, the rough approximation seems to be sufficient to explain the observations. It might be interesting to see whether there was any difference between the superficial friction of a gas and a liquid which did not evaporate, and of a vapour in contact with its own liquid. In one case there would be no exchange of molecules between the two bodies that were sliding past one another, while in the second case there would be an exchange. A study of the conduction of heat between a gas and a liquid might also help to elucidate the nature of the exchange which takes place between a liquid and its vapour.

In the paper on the vapour pressure of mercury, there are some very rough approximations which are hardly sufficiently accurate for general application. One is as to the extent to which a saturated vapour obeys the laws of a perfect gas. Hertz assumes that this is more nearly true the lower the temperature. This is not generally so. For each kind of material there is a particular temperature at which its saturated vapour most nearly obeys these laws, and below as well as above this temperature it departs from these laws. Again, there is a process, described at the bottom of p. 203 and top of p. 204, which cannot possibly be carried out. He says: "Let a quantity of liquid at temperature T be brought to any other temperature. At this temperature it is converted into vapour without external work." This is absolutely impossible, and the equation he deduces from all this is not true, though it is sometimes a rough approximation to the truth.

There is a very interesting paper on the floating of bodies by thin sheets of rigid material like ice. Hertz shows that if the sheet be large enough it would be possible to cause a thin sheet of iron, which by itself would sink, to float by placing weights at its centre. The weights might so depress

the centre and make the sheet so boat-shaped as to float both themselves and it.

In 1883 Hertz published a deduction from first principles of Maxwell's equations for the electromagnetic field in the symmetrical form, afterwards used by himself in his investigations on oscillatory discharge waves. He applies the very same arguments by which Helmholtz, Lord Kelvin, and others had argued, from the work done by one electric current on another, that there must be a corresponding reaction of the second on the first current, and hence deduced electromagnetic induction. Hertz applies this argument to the case of a ring magnet changing in strength and producing magnetic force on another ring magnet in its neighbourhood, and doing work there, and shows thereby that there should be a magnetic force due to a changing electric field exactly corresponding to the electric force due to a changing magnetic field. This, of course, is what Maxwell describes as the magnetic effect of the changing electric displacement, and its effects are expressed by the very same equations as Maxwell deduces. The argument is no more and no less conclusive than in the corresponding application of the principle of the conservation of energy to deduce ordinary electromagnetic induction. Hertz is careful to point this out, for he was early imbued by Helmholtz with the fact that the principle of the conservation of energy is by itself utterly inadequate as a complete explanation of physical phenomena. He specially mentions himself Helmholtz's interest in this problem of the simplest basis for dynamics, and Hertz's last great work was to place general dynamics on a sound basis. The simplest of all cases is the easiest in which to see how the principle of the conservation of energy fails to give a complete solution. A body moving without any action from other bodies describes a right line at a constant velocity. The principle of the conservation of energy requires the constant velocity. But, why the right line? Conservation of energy cannot solve even the simplest of all examples. It would be well if some modern chemists would mark, learn, and inwardly digest this.

The part of Hertz's work which is of greatest interest just now is that in connexion with kathode rays. He began with some very interesting observations on the aura accompanying spark discharges. It appeared to be projected from the positive electrode, and occasionally formed a vortex ring of incandescent gas, which lasted for an appreciable time between the electrodes of a jar discharging in air. Goldstein has noticed similar effects; and some recent experiments on the discharge of large Leyden batteries, in which some of the phenomena of globular lightning seem to have been reproduced, make it appear possible that this latter is a spherical vortex of incandescent air.

Hertz's study of kathode rays, in 1883, set finally at rest two questions. In the first place he showed that the discharge in a gas may be as continuous as any other form of current. In no case are we absolutely certain that the current is absolutely continuous. On the large scale it certainly is; but all we know of electrolysis seems to show that on a sufficiently small scale the current is carried in detachments, and is consequently essentially discontinuous. This, however, was not the question at issue, and so far as a continuity of the same kind as that in any liquid electrolyte is concerned, Hertz showed that the discharge through a gas might be equally continuous. The second question he decided was as to the direction of flow of the average current in an exhausted space. He showed that the average flow at any point was nearly the same as if the whole space were a conductor: that there was no connexion between the kathode rays and the flow of the current. From experiments on kathode rays projected down a tube, and quite away from both electrodes he deduced that they produce no magnetic action outside the tube, although they are deflected by the magnet. His conclusion, that the kathode rays are not streams of electrified particles, was largely founded on this, and on another experiment on the action of electrostatic force on the particles. This experiment on the magnetic action of kathode rays is quite inconclusive, and it is very remarkable that Hertz

should have attributed much importance to it. Whatever current was carried down his tube by the kathode ray must have come back the tube by the surrounding gas, and these two opposite currents should have produced no magnetic force outside the tube; and this is exactly what Hertz observed. In a similar way, what he observed in the case of a flat box was the average direction of the current, and he showed that this average direction was approximately the same as in a conducting sheet. This proved that if there were any concentration of the current along the direction of the kathode rays, this concentration was neutralised by a corresponding return current, so that the average current was as described. At the same time there does not seem much doubt but that the kathode rays only carry a very small part of the current. The third part of the paper is concerned with the electrostatic effects due to kathode rays. The experiments do not seem to fully justify the conclusion drawn, that kathode rays cannot be charged molecules. Sufficient account does not seem to have been taken of the shielding action of the conducting gas surrounding the kathode ray, nor of the way in which the potential is distributed between two electrodes in a gas. Hertz describes an experiment with two plates inside the tube kept at a considerable difference of potential. He says :— "The phosphorescent image of the Ruhmkorff coil discharge appeared somewhat distorted through deflection in the neighbourhood of the negative strip; but the part of the shadow in the middle between the two strips was not visibly displaced." Now this is exactly what one might expect, because the fall of potential between two such strips is very small indeed, except close to the negative strip, and there the electric force *did* deflect the rays. Hence the conclusion is just the reverse of the one Hertz gives. From the experiment it appears that kathode rays do behave like electrified particles. It is very remarkable that in all these investigations Hertz does not once even mention, as a thing to be explained, the repulsive actions which Crookes observed, and

which have been almost universally attributed to the impact of gas particles.

The other important paper, on the transmission of kathode rays through thin metallic films, is particularly interesting as the starting-point for Lenard's work, which has resulted in the discovery of the X-rays. A good deal of what Hertz observed would be accounted for by the production of X-rays where the kathode rays meet the diaphragms, and by the reproduction of kathode rays mixed with X-rays on the other side of the diaphragm, which would thus act as a sort of local electrode. That something exists in a vacuum on the far side of such a thin film, which does not ordinarily exist in X-rays in air, seems conclusively proved by there being something there which can be deflected by a magnet. There seems no doubt that kathode rays themselves are quite invisible, and that it is only where they are interfered with by gaseous molecules or by phosphorescent solids that they are sources of light. This is very much what one would expect. An electrified atom would not in general be a source of light unless its free movement were interfered with by impact.

The concluding article, on his master Helmholtz's seventieth birthday, is a noble and generous tribute to that great teacher's abilities and character. How truly he portrays the important characteristics of a University Professor! "It is true that Helmholtz never had the reputation of being a brilliant university teacher, as far as this depends upon communicating elementary facts to the beginners who usually fill the lecture-rooms. But it is quite another matter when we come to consider his influence on trained students, and his pre-eminent fitness for guiding them in original research." The most important duty of a University is to increase the knowledge of mankind, and to train up a new generation who may be able to continue the good work. It is thus mankind has advanced since the dawn of civilization in Egypt. He who produced the most enthusiastic disciples has most advanced the well-being and the well-living of the race.

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ON THE EFFECT OF PRESSURE IN THE SURROUNDING GAS ON THE TEMPERATURE OF THE CRATER OF AN ELECTRIC ARC. CORRECTION OF RESULTS IN FORMER PAPER.

[Conjointly with W. E. Wilson, F.R.S. From the *Proceedings of the Royal Society*, Vol. 60. Read December 17, 1896.]

In May 1895, a preliminary paper by one of the authors was read at the Royal Society, in which is described the apparatus used for these experiments, and the results which were then obtained.

The primary object of this research was to determine, if possible, whether the temperature of the crater in the positive carbon varies when the pressure in the surrounding gas is changed.

It has been suggested that the temperature of the crater is that of boiling carbon. The most modern determinations give this temperature of the crater as about 3300-3500° C.*

If this is the true boiling point of carbon, it is then clear that solar physicists must find some other substance than solid carbon particles to form the photospheric clouds in the Sun, as the temperature of this layer is most probably not below 8000 C.,† unless, indeed, the pressure in the solar atmosphere is sufficient to raise the boiling point of carbon to about this temperature (see p. 448). It is in order to throw some light on this subject that these experiments were undertaken.

* Wilson and Gray, "Roy. Soc. Proc.," vol. 58; Violle, "Journ. de Phys.," 3rd series, vol. 2, 1893, p. 545.

† Wilson and Gray, "Phil. Trans.," A, vol. 185, 1894.

The gas used in our first experiments was nitrogen, and we found that the radiation from the crater fell off in a most remarkable manner whenever the pressure was raised in the box surrounding the arc. This falling off was not due to any very large extent to visible cloud or smoke, and the crater seemed so much reduced in temperature as to glow with only a red heat. This seemed to show that the temperature of the crater depends on how much it is cooled by the surrounding gas, and not on its being the temperature at which the vapour of carbon has the same pressure as the surrounding atmosphere.

It was found that we were limited to pressures not exceeding about 20 atmos., as at this pressure we could not withdraw the negative carbon sufficiently to see into the crater without the arc breaking. We were then only able to obtain a current from a battery of accumulators which had an E.M.F. of 110 volts. Since then we obtained a Crompton dynamo which could give 300 volts and 15 ampères, and which was driven by a turbine.

From the great difficulty of obtaining a sufficient quantity of pure nitrogen under pressure, we obtained a 20 ft. cylinder of air compressed to 120 atmos. With this we tried a series of experiments, and these at first seemed to corroborate our former ones, in which we used nitrogen; but we found that at any rate some of the radiation, and possibly a great deal of it, was cut off by the formation of what appeared to be red fumes of NO_2. We found no absorption from this cause so long as the pressure was nearly atmospheric, but at about 100 lbs. pressure this gas was formed with great rapidity, and undoubtedly cut off a great deal of the radiation. We easily confirmed our belief in the presence of this gas by its well known absorption spectrum.

Lest heat dissociation might cause an apparent increase in the amount of NO_2, we tried heating some of this gas in a flask. We observed that when hot the brown fumes became golden yellow, and the absorption bands nearly disappeared, so that the heating could *not* have been the cause of the apparently enormous production of NO_2 at high pressure.

We next tried whether oxygen blown into the arc would

burn up the carbons, but found it did not do so to any serious extent, and so tried the arc in a compressed atmosphere of this gas.

The arc burned very nicely indeed in the oxygen, the carbons keeping a good shape, and a very steady crater. The oxygen was, however, so contaminated with nitrogen that at high pressure enormous quantities of NO_2 were again formed, so that we could not proceed further with the radiation experiments. The arc was a bright blue bead, about the size of a pea, and the spectrum was a beautiful banded one.

From these results we concluded that the reduction of radiation, and red-hot appearance of the crater in the former experiments in nitrogen, were due to its being contaminated with oxygen and to the large quantities of NO_2, which were formed by the arc when under pressure.

We next tried the arc in hydrogen. The gas was obtained as pure, but contained hydrocarbons as an impurity, possibly from having been compressed into a cylinder which had previously been charged with coal-gas.

The arc in hydrogen at atmospheric pressures was a long, thin flame, that moved as far up the carbons as possible; especially on the negative carbon it walked up a cm. along the cone. It went so far that it fused the copper ring that held the negative carbon, and we had to replace it by an iron wire lashing. It was very unsteady, and trees of soot and a deposit of hard graphitic carbon formed on this positive carbon as if there were electrolysis of the hydrocarbon, and carbon were electro-negative compared with hydrogen. This growth took place all round the crater, while there was no tendency for anything to grow on the negative carbon.

The arc was only 5–6 mm. wide, and sometimes over 2 cm. long. There was a green outer flame, with a bright red line not a mm. wide down the middle of it. Where it impinged on the negative carbon there was a bright red flame from the middle of the bright spot on the carbon. The outer greenish part seemed to give much the same spectrum as the green cone in a

Bunsen burner, while the red flame and line was undoubtedly glowing hydrogen. As we saw the C and F hydrogen lines very distinctly, the red C line being dazzling bright and not nearly so wide as in a coil spark at atmospheric pressure, whenever the image of the red part of the arc was thrown on the slit of the spectroscope, the appearance was quite like that of a Solar prominence.

The end of the positive carbon was pitted into a number of craters, as the arc was very unsteady; and when the pressure was raised it was almost impossible to keep an arc going, partly because the arc broke when it was elongated the least bit, and partly because a complete lantern of soot trees grew all round the crater, and seemed to short-circuit the arc from time to time.

The arc being very unsteady, no satisfactory reading of the voltage and current was possible. At from 60 to 80 lbs. pressure the voltage varied from 60 to 80, and the ampères kept continually varying from 15 to 20. At 40 lbs. with 20 ampères the volts varied from 50 to 60. The crater was not well developed, so that the radiation observation, even at low pressures, was not very satisfactory, while at high pressures the arc was too short to see into the crater at all, and the lantern of soot trees hid a considerable length, 3 or 4 mm. of the negative carbon besides. The radiomicrometer gave 440 divisions with a good arc in air, and 380 with the moderately good crater in hydrogen. But this difference is no greater than would often occur with a good and moderately good crater, so that there is not any proof of a difference of temperature due to cooling power of hydrogen. These experiments showed us that it was quite hopeless to get any measures of radiation under pressure with hydrogen.

We finally tried an atmosphere of carbon dioxide. We used a cylinder of liquid CO_2, which was connected to our arc box by a copper tube and stop valve. The arc burned fairly well in this gas, and, except for the difficulty of getting a sufficiently long arc at pressures above 150 lbs., some pretty satisfactory measures of radiation were obtained. We found that whenever the

pressure was suddenly reduced, there was a fog formed in the box, which cut off the light enormously. Also by looking down the steel tube, which is closed at its end by a lens, we could see powerful convection currents in the gas which scattered a lot of light. At high pressure the refraction due to these currents prevented any sort of an image of the crater being formed while the pressure was varying. While the pressure was steady a good image could be formed. This tube is nearly 3 ft. in length, and only $\frac{1}{2}$ in. in bore, and it would naturally take time for the gas to settle down throughout its length. We propose to have this tube removed, and the aperture in the box closed by a strong piece of plain glass, and to form an image of the carbons by a lens placed at a suitable distance outside. This we expect will remove the difficulty arising from these convection currents.

The result of all these experiments so far is that it would require more evidence than we have been able to get, to affirm that the temperature of the crater of the arc is either raised or lowered by pressure. We got some very concordant observations, which showed the temperature to be lowered with pressure, and in which at the time we could see no evidence of absorption by fog; but then, at other times, there was undoubtedly absorption from this cause. We certainly got no evidence that there is any appreciable increase of temperature. When the arc was started in the gas at a low pressure and then the pressure was raised, the radiation at the low pressure was greater than at a high pressure; but when the arc was started first in the gas at high pressure, and then the pressure reduced, the radiation was rather higher in the gas at high pressure. From all this we concluded that the greater part of the differences we were observing were due to the absorption of the light in the long tube already mentioned, which increased the longer the arc was kept burning, and was probably greater at high than at low pressures. The best observations were made with variations of pressure from 15 up to 100 lbs. per sq. in., and there seems very little evidence of much change of radiation with this change of from 1 up to between 6 and 7 atmos.

The whole question is surrounded with great difficulty. If the carbon be really in equilibrium with its own vapour at the temperature of the crater and at the pressure of the surrounding atmosphere, some relation must exist between the change in pressure and change in temperature of the crater. If we knew the latent heat of volatilization of carbon, we should be able to calculate the change of temperature from the well-known thermodynamic formula.

$$\frac{\delta T}{T} = \frac{\Delta v}{\lambda} \delta p.$$

Δv can certainly be approximately determined on the supposition that the absolute temperature of the crater is fourteen times the absolute temperature of the freezing point, *i.e.* 3800. We thus get for gaseous carbon $\Delta v = 10^4$, *q.p.*, at this temperature. For 1 atmos. $\delta p = 10^6$, *q.p.*, so that

$$\frac{\delta T}{T} = \frac{10^{10}}{\lambda}.$$

Hence, unless the latent heat of carbon be enormously great compared with that of other substances, $\delta T/T$ will be considerable. If λ be as great as the latent heat of vaporization of carbon given by Trouton's law, *i.e.* about 4000 calories, or $16{\cdot}8 \times 10^{10}$ ergs, $\delta T/T$ would be about $\frac{1}{17}$, and δT would be nearly 220° C. for each atmosphere, and a change of pressure of about 18 atmos. would raise the temperature of the crater to that estimated for the Sun. The corresponding increase of radiation would be very great, for the radiation varies, at least approximately, as the fourth power of the absolute temperature. This would lead one to expect that the radiation would be nearly doubled for each 4 atmos. added. Such an increase as this certainly does not take place, so that we may conclude that either the temperature of the crater is not that of boiling carbon, or else that the latent heat of volatilization of carbon is very considerably greater than that calculated from Trouton's law. Even though this latent heat were as great as the heat of

combustion of C to CO_2, *i.e.* 7770, there would be an increase of about 70 per cent. in the radiation for an increased pressure of 6 atmos. Such an enormous latent heat is unprecedented, and yet our experiments would, almost certainly, have shown such an increased radiation as this. So far, therefore, the experiments throw considerable doubt on the probability that it is the boiling point of carbon that determines the temperature of the crater. It might be questioned whether there is energy enough in the current to do all this work; but upon an extravagant estimate of the amount of carbon volatilized in the crater, it appears that there is more than a hundred times as much energy supplied by the current as would be required for volatilizing the carbon, even though its latent heat were as great as the heat of combustion of C into CO_2.

There is another considerable difficulty in the theory of the temperature of the crater being that of boiling carbon, arising from the slowness of evaporation. The crater on mercury is dark; but then it volatilizes with immense rapidity and the supply of energy by the current being more than 100 times that required merely for evaporation, there seems very little reason why even a considerable difference in latent heat should make any sensible difference in the rate of evaporation of mercury and carbon, especially as, at the same temperature, the diffusion of carbon vapour is nearly three times as fast as that of mercury vapour and the temperature immensely higher.

We would, in conclusion, call attention to a cause of opacity in the solar atmosphere that is illustrated by the effect of convection currents in the long tube we were observing at high pressures; these convection currents behaved just like snow, or any other finely divided transparent body immersed in another of different refractive index. Light trying to get through is reflected backwards and forwards in every direction, until most of it gets back by the way it came. The consequence was that even the electric arc light was unable to penetrate the tube at high pressure, when these convection currents were active. The only light that came out of the tube was the feeble light

outside, which was returned to us by reflection at the surfaces of these convection currents. In a similar manner we conceive that any part of the solar atmosphere which is at a high pressure, and where convection currents, or currents of different kinds of materials, are active, would reflect back to the Sun any radiations coming from below, and reflect to us only the feeble radiations coming from interplanetary space. In his paper on "The Physical Constitution of the Sun and Stars" (Roy. Soc. Proc., No. 105, 1868), Dr. Stoney called attention to an action of this kind that might be due to clouds of transparent material, like clouds of water on the Earth, but in view of the high solar temperature it seems improbable that any body, except perhaps carbon, could exist in any condition other than the gaseous state in the solar atmosphere; so that it seems more probable that Sun-spots are due, at least partly, to reflection by convection streams of gas, rather than by clouds of transparent solid or liquid particles.

82

DYNAMICAL UNITS

[From *Nature*, February 16, 1897.]

MANY of the writers of letters on this subject seem to have forgotten that the question Prof. Perry raised was as to the best system of units to use with a class of *engineering students*. This question is very seriously complicated by the fact that all their books, and almost all their teachers, use a system of units which is *not* that of the poundal, but is essentially the one that Prof. Perry advocates. This is a very serious fact that every teacher of engineering students must take account of, and the question is, "What system shall the teacher use with engineering classes?" I entirely agree with Prof. Perry in thinking that it is much better for the teacher to accommodate himself to the requirements of his class, than for him to force his class to use one system when working for him, and another outside his class-rooms. This latter plan tends to perpetuate the prevalent notion that science has nothing to do with practice.

As regards the question of why students find dynamics and the notion of mass in particular so difficult, I do not believe that this is due to any difficulties about various systems of units. In matters upon which their ideas are clear and distinct, such as length and time, the existence of different units,—feet, yards, miles, etc., minutes, days, etc.—presents very little difficulty. To British students these varieties of units in which to measure the same quantity are so familiar that they naturally look upon varieties of units with contempt. It is

only when the thing measured is not clearly and distinctly conceived that confusion and all sorts of difficulties arise. Hence the importance of getting students actually to come into contact with the things themselves. Until a student has some ideas of density, acceleration, etc., as things to be measured, he will be quite certain to misapply the rules he has learnt for dealing with the black marks he makes on a piece of paper, and which he calls by their names. Now of all these dynamical quantities, of which students are generally expected to form clear and distinct ideas without any actual experience of the things themselves, the most abstruse, and the one about which the most metaphysical statements are made, is "quantity of matter" or "mass." *A priori*, there is no way by which we can determine whether a quantity of gold is equal to a quantity of iron. In ordinary practice there are two kinds of equality which are commonly used: volume and weight. If I tell any ordinary man to mix equal quantities of whisky and water, he will mix equal volumes. It is quite as common to mean equal volumes as equal weights by equal quantities in common language. When a student is told, as an *explanation* of the word "mass," that it means "quantity of matter," there is an appeal made from the obscure to the more obscure. It is a case of huggermugger. The student thinks the teacher must have some clear and distinct idea of what he means by "quantity of matter," and is ashamed to say that to him it is no explanation of mass to call it "quantity of matter." Thus begins the demoralisation of the student. He is demoralised by having to swallow undigested a term of which neither he nor his teacher has a clear and distinct idea, and he naturally concludes that the whole subject is one that "no fellow can understand."

If teachers and books would give up this metaphysical notion of "quantity of matter," and would deign to confine their attentions to actually measureable quantities, like volume, weight, and inertia, the student could be given, by making experiments, clear and distinct ideas of these properties of

matter as actual quantities to be measured. Once he had these clear and distinct ideas, a variety of units for measuring each of them would not present any serious difficulty. That the inertia of matter is proportional to its weight, that the inertia of a body is the same here as in the Moon and Jupiter—these are most important physical facts to be proved by experiment, because we have no other way of ascertaining them. It is often *assumed* that the inertia of a hot body is the same as of the same body when cold; but I do not know of any accurate experiments having ever been made to prove it, and I am quite certain that a great deal too little is known of the structure of matter, and of its relations with the ether, to be able to prove *a priori* that the inertias are the same. The suggestion would probably involve the further suggestion that we could make a machine to utilize some of the energy in the ether; but does any one profess to know so much of that remarkable thing as to be quite certain that this is impossible? Any way, there is no doubt that to a considerable degree of accuracy inertia *is* a constant property of a body, and equal inertias may consequently be very reasonably considered as equality of such a very important property of two bodies, that scientific people are justified in their shortly describing the bodies as equal, which is what they usually do, and is all that they can really mean when they speak of equal quantities of iron and gold. Why, then, trouble unfortunate students with the idea that there is some huggermugger metaphysical "quantity of matter" called "mass," of which they are supposed to have a clear and definite conception distinct from this equality of inertia? Why not call it inertia when it is inertia that is meant, and drop out of use that word "mass," round which such a tissue of indistinct and obscure ideas have grown, that it is almost hopeless to separate it from them?

I hope some word more euphonious than "slug" will be found for the unit of inertia on the engineer's system. I would suggest "ert" as a term that would easily recall the quantity inertia.

[From *Nature*, March 11, 1897.]

Within the moderate dimensions of a letter it is hard to give due weight to every aspect of a complicated matter, and while trying to emphasise one side I have somewhat overstated the case, as is evident from the way in which Prof. Lodge has taken me up. I was only considering the teaching of elementary dynamics to engineering students. I do not object to a teacher explaining that inertia is such an important constant property of matter, that equality of inertia is our *definition* of equal quantities of matter. What I do object to is a common inversion of this, by which equality of inertia is *explained* by saying that the quantities of matter are equal. In addition, I urge that teachers of elementary *dynamics* should call what is usually called mass, inertia, so as constantly to bring before the student the fact that this is *the* property with which the dynamics of motion deals. I do not plead guilty in this to confusing the issues. The issues of Prof. Perry's review have been overlaid with a discussion as to one of the greatest advances of modern physics, namely the possibility of representing physical quantities by algebraic symbols; but I was trying to recall the original issue, as to the way dynamics should be taught to engineering students. Babes must be treated babyishly, and as long as engineering students are what they are now, and have to attend a variety of lecturers, and read engineering books as they are, I agree with Prof. Perry in recommending that the engineer's unit of inertia be used by their teachers. I have already explained that a multiplicity of units is a very minor difficulty to those who have once grasped what it is that is being measured; but I do think it confuses them, while getting these ideas, for one teacher to use one system, and another another system, and for each teacher to call the system of the others by hard names.

83

CATHODE RAY EXPERIMENTS

[From the *Electrician*, June 4, 1897.]

As a contribution to the possible elucidation of Mr. Swinton's interesting and beautiful experiments on cathode rays, described to the Royal Society on March 11th, and published in the *Electrician* of April 23rd last (page 868), I would suggest they are largely due to the difficulty of supplying gas molecules sufficiently rapidly to keep the whole surface of the cathode discharging. After the first discharge the supply of fresh molecules would largely take place by diffusion from the edges, the gas in front having been swept away by the discharge. As fast as the gas diffused in from the edges it would be driven off by the discharge, and, in consequence, the discharge might become a hollow cone as Mr. Swinton observed.

In the case of the interposed sector, it would interfere with the supply of gas over a considerably larger area than it actually covered, when close to the cathode, and this again is in accordance with Mr. Swinton's observation.

I would further suggest that some of the penetrating power of cathode rays in gases may be due to the sudden impulse over a restricted area producing something like a vortex ring, which is shot out into the surrounding gas. Such a motion might penetrate considerable distances, and may be connected with Righi's extraordinary experiments on phenomena analogous to globular lightning as generally described. Any wave motion of very short wave-length would be propagated in rays, and if

very intense and accompanied by a powerful electric force, might be the means of carrying an electric charge forward along with the intense compressions, the compression and resulting high temperature enabling the charge to be transferred from molecule to molecule.

These suggestions are all rather wild and not carefully digested, but between all the various suggestions of various authorities we may hope for light on these very obscure phenomena, while we can expect no help from bottled-up wisdom, however profound. May this be my excuse for making these crude suggestions.

84

ZEEMAN'S PHENOMENON

[From *Nature*, September 16, 1897.]

IN Section A at the Toronto meeting of the British Association, Dr. Lodge raised the question as to whether we should expect on a simple theory that the spectral lines should be simply widened or be doubled by magnetic force. The simple theory depends on the acceleration or retardation of electrons performing circular orbits under the action of a magnetic force normal to the plane of the orbit. If the plane of the orbit be not normal to the magnetic force, it might appear that the acceleration or retardation would be only that due to the component of the force normal to the plane of the orbit. From this it would follow that the lines would be *widened* and not *doubled*, because every intermediate acceleration or retardation would occur between the extreme cases of orbits parallel and perpendicular to the magnetic force.

This suggested theory, however, overlooks the effect of the magnetic force in altering the plane of the orbit. The complete theory can be very much more simply obtained by another method of attack. The motions, being assumed simply periodic in the undisturbed motion, can be resolved for each electron into three linear vibrations, two at right angles to the magnetic force and one parallel to it. This latter is undisturbed and gives no light in the direction of the magnetic force. Each of the other linear vibrations is disturbed, and we can easily see how by considering that a linear vibration may be considered as due to two circularly polarized vibrations.

Each of these component circular vibrations will be altered by the magnetic force normal to its plane, one being simply accelerated and the other retarded. We can consequently see that this more complete theory leads to the conclusion that the lines would be *doubled* and not *widened*, though, of course, they may be also widened owing to other disturbances of the motion. There would be no difficulty in writing down the equations of the resulting motion of the electron;* but it seems hardly necessary to do so, as this geometrical analysis leads to the kind of vibration emitted, which is all that we can observe.

[* Cf. Larmor, *Phil. Mag.*, Dec. 1897, or "Æther and Matter," p. 341; also 9 *infra*.]

85

CHANCE OR VITALISM

[From *Nature*, October 6, 1898]

I AM glad to see that Prof. Karl Pearson has called attention to Prof. Japp's address at Bristol. Only that one does not like to criticise adversely a presidential address, I would at the time have pointed out the weakness in the argument that Prof. Pearson criticises. He does not go nearly so far in this criticism as the circumstances warrant. It is conceded that right- and left-handed crystals of quite sensible size are produced sufficiently separated to be seen and handled as separate crystals. Now assuming, what there is every reason otherwise to think quite probable, that life started from some few centres, the chances are, not that it was equally divided between right- and left-handed forms, but that one or other of these forms preponderated. In fact, if life started from a single centre, it *must* have been either right- or left-handed. Hence the fact adduced only shows what was otherwise very probable, that life started from a small number of origins, possibly only one.

Another reason for either a right- or left-handed structure in living organisms on the Earth, and one which diminishes the force of the foregoing argument for a small number of origins, is that it probably started either in the northern or in the southern hemisphere, and in either case the rotation of the Sun in the heavens may be a sufficient cause for a right- or left-handed structure in an organism growing under its influence.

86

ASYMMETRY AND VITALISM

[From *Nature*, November 24, 1898.]

PROF. JAPP has so entirely changed his position that it is useless to attempt to follow him. I would desire, however, to correct misunderstandings into which he has fallen in respect of my contentions with reference to his original position.

I did not intend to suggest that life originated in a crystalline form; but merely that, as living things can now assimilate crystalline bodies, the first living organism may have originated in connexion with and by utilising a crystal, and that the asymmetry of this original living organism may have been controlled by the accidental asymmetry of the original crystal.

Once life began, I presume it descended, as it does now, by section and so forth; and, as I cannot follow Prof. Japp's difficulties as to a particular asymmetrical system breeding the like, I cannot see how the intervention of intelligence is required for its propagation, any more than for the growth of a particular asymmetrical crystal, once it is started.

This preponderating influence of the parent entirely explains the other misunderstanding Prof. Japp has fallen into. I never suggested that the rotation of the Sun, probably a very feeble cause, could make a seed, with its impressed asymmetry, grow into a tree with a different asymmetry merely by bringing the seed from the northern to the southern hemisphere. All I suggested was, with reference to Prof. Japp's original position,

that at the *origin* of life the first living organism may have been given a particular asymmetry by its having been produced in one or other hemisphere. A cause which may have been quite sufficient to give this asymmetrical bias during the time of origination, may be quite inadequate to produce a change in the bias once it has been given.*

87

DIFFUSION IN RELATION TO WORK

[From *Nature*, November 10. 1898.]

IN this month's Philosophical Magazine Mr. A. Griffiths has an interesting paper on diffusion convection, in which he suggests an indirect method of measuring rates of diffusion of liquids, and concludes with the following deduction from the fact that diffusion sometimes produces convection currents and sometimes does not:—"Does not this indicate that the heat produced on mixing a solution with water depends on how the mixing takes place? Is the matter connected with a sort of surface-tension existing in the spaces between a strong and a weak solution?"

Mr. Griffiths does not seem to have observed that his investigation applies quite well to gases as to liquids, and that his indirect method of measuring rates of diffusion is applicable to gases. In the case of gases there can be [no] sensible surface-tension, and, as the theory of diffusion in gases is quite simple, there is no serious difficulty in seeing how there is a difference between different ways of mixing them.

[* The products of the two hemispheres are now alike as regards asymmetry, spirality of tendrils in plants, etc.]

It is generally known that two different gases may be mixed either by irreversible or by at least partially reversible processes. They may be allowed to diffuse freely into one another, or may be separated by a porous partition. In the latter case a considerable difference of pressure may be produced between different parts of the space containing them, and this difference of pressure can be used to do work. The final condition in this case is, of course, cooler than if the gases did no external work. In the same way a solution diffusing into water may do so without doing external work, or it may do so by a reversible process through a semi-permeable diaphragm, producing considerable differences of pressure, which may be used to do work. The final condition in this latter case would, of course, be cooler than in the former case of inter-diffusion without doing external work. Now whenever convection currents are produced, these are to some extent reversible. We might put vanes into the liquid to be moved by the currents and to do work outside the liquid, and by reversing this we would reverse the convection currents. Hence any method of mixing in which convection currents are produced, which do work or produce heat outside the liquid, will necessarily produce less heat in the liquid than a method of mixing in which there are either no convection currents, or these produce heat by viscous flow inside the liquid.

That we can, at pleasure, either use the diffusion of two gases into one another to do external work or not, is really not different from the case of a single gas expanding into a larger volume. We may do work by this expansion and cool the gas, or we may allow the gas, as in Joule's experiments, to expand into a larger volume without doing external work, and in this case there is only a very small change of temperature.

In these cases it is a question of change of entropy in the system, which can either be effected by an irreversible process in which no work is done, or by a variety of other processes more or less reversible, in which the more reversible they are the more work can be done. In the case of producing convection currents, or, in general, of diffusion of a heavy fluid upwards

into a lighter one, the amount of heat produced would not be exactly the same as if gravity were not acting: the centre of gravity of the system is raised by diffusion. Now in Mr. Griffiths' case, and in the case of diffusion currents generally, this raising of the centre of gravity takes place throughout part of the space considered, by diffusion, and the centre of gravity is continually falling down again in the convection currents. Hence the work that can be done by the convection currents is part of the work that was done by diffusion against gravity. In the case of diffusion without convection currents, we might use the whole of this work done against gravity, by which the centre of gravity of the system has been raised, to do external work. If, for example, the containing vessel were supported at its centre of gravity, in the unmixed condition, the centre of gravity would, after diffusion, be above the point of support, and the vessel and its contents might be arranged to turn round the support doing work during the fall of the centre of gravity to its original level. Another way of utilising the rise is to allow the fluid to flow into another broader vessel until its centre of gravity has returned to the original level. The thing to be specially observed is, that the amount by which the centre of gravity is raised depends entirely upon the shape of the vessel. If it be tall, the centre of gravity will be raised a great deal; while if it be low, the centre of gravity will be only slightly raised. By causing diffusion to take place in a tall thin vessel the final temperature will be lower than in a broad low one, not on account of any superficial tensions, but on account of the work done against gravity. In Mr. Griffiths' method diffusion is continually taking place along tall thin vessels, with convection currents lowering the centre of gravity again by flow into broad ones.

88

NOTE ON THE CONNEXION BETWEEN THE FARADAY ROTATION OF PLANE OF POLARIZATION AND THE ZEEMAN CHANGE OF FREQUENCY OF LIGHT VIBRATIONS IN A MAGNETIC FIELD.*

[From the *Proceedings of the Royal Society*, Vol. 63.
Received March 2, 1898.]

THE rotation of the plane of polarization of light in a magnetic field is due to the velocity of propagation of light circularly polarized in one direction being different from that of light circularly polarized in the opposite direction. The Zeeman effect is due to a difference in the frequency of vibration of these circularly polarized waves. What is required is to connect the frequency of vibration with the velocity of propagation.†

All modern theories of dispersion connect these two quantities. The velocity of propagation of light in transparent media is now universally considered to be determined in part by what

* Being notes of a contribution to the discussion of Mr. Preston's paper, read January 20, 1898.

† From an abstract of a paper by M. Becquerel, in the *Comptes Rendus* of last year, I understand him to view the Faraday effect as due to a carrying round of the light vibrations by matter rotations. This is quite in accordance with the view sometimes held as to refraction, namely, that it is due to the waves being propagated through the molecules more slowly than through the ether. The dynamical theory of these views is difficult on account of the smallness of the molecules in comparison with the length of the waves. M. Becquerel's view, as I understand it, makes the Faraday effect depend on a change of frequency of rotation of the waves in matter rather than on their velocity of propagation, and is consequently quite at variance both with the commonly received theory and with the one put forward in this note.

may be called the syntony of the matter and light vibrations, and is consequently dependent on the frequency of the matter vibrations. In most substances the dispersion is controlled within the visible spectrum by a great absorption band in the ultra-violet, this band representing a possible frequency of vibration of the molecules, *i.e.* of that part of the molecules which affects the ether, be it electrons or something which simulates the actions ascribed to electrons. If owing to any cause this absorption band be changed in position, *i.e.* the frequency of the molecular vibrations be altered, the dispersion of the medium will be changed, and with it the velocity of propagation of light within the visible spectrum. Now Zeeman has shown that in a magnetic field the frequency of vibration of molecules *producing* light circularly polarized in one direction is different from that of molecules *producing* light circularly polarized in the opposite direction, and that consequently the absorption bands for molecules in a magnetic field for oppositely circularly polarized waves will differ. Hence we conclude that the velocities of propagation of oppositely polarized waves within the visible spectrum will differ, and that is the Faraday effect. Hence these two phenomena are directly connected with one another, independently of any other than general dispersion theory, *i.e.* independently of any theory of electrons, such as Lorentz has shown will explain the Zeeman effect. If we introduce such a theory, and use it to explain the Faraday effect upon the lines now laid down, we arrive at the interesting conclusion that before the Zeeman effect was observed the Faraday effect would have shown that in the majority of substances we must assume the ether vibrations to be due to the motion of a *negative* electron. In substances with a negative Faraday effect, such as some magnetic bodies, we may conclude either (*a*) the ether vibrations are due to a *positive* electron, or (*b*) the absorption band controlling the dispersion is in the ultra-red, or (*c*) the cause of the Faraday effect may be due rather to a difference of *intensity* of the absorption band for oppositely circularly polarized vibrations than to a difference

of *frequency*. This latter alternative is in some ways the most consonant with the usual theory as to the difference between paramagnetic and diamagnetic bodies.

If we proceed to calculate what amount of Faraday effect might be expected from the observed value of the Zeeman effect, we are met by the difficulty of obtaining data. We require to know, for some one or more substances, the amount of the Zeeman effect, the amount of the Faraday effect, and the ordinary dispersion of the substance for light. The only gas (and it is only in gases that the Zeeman effect has been observed) for which the dispersion and Faraday effect are well ascertained, is air, and for it the amount of the Zeeman effect has not been published. In preparation, however, for the data, it may be interesting to write down shortly the equations which, upon Lorentz's theory as to the cause of the Zeeman effect and Larmor's theory as to dispersion, we may expect to hold.*

Assuming, then, that the electric displacement in the medium is partly due to the electric force and partly to the displacement of electrons, we may write for its components in the wave face supposed plane and perpendicular to $z = 0$,

$$f = \frac{K}{4\pi} P + Nex, \quad g = \frac{K}{4\pi} Q + Ney,$$

where P, Q are electric force, e the electron, and x, y are coordinates that measure the displacement of this latter. From these and the usual equations connecting magnetic and electric force† we get for the medium

$$K\ddot{P} + 4\pi Ne\ddot{x} = \frac{d^2P}{dz^2}, \qquad K\ddot{Q} + 4\pi Ne\ddot{y} = \frac{d^2Q}{dz^2}.$$

* Larmor, *Phil. Trans.* A, 1897.

[† Namely, the circuital relations which in this case become

$$4\pi u = -\frac{d\beta}{dz}, \qquad 4\pi c = \frac{d\alpha}{dz},$$

and

$$-\frac{d\alpha}{dt} = -\frac{dQ}{dz}, \qquad -\frac{d\beta}{dt} = \frac{dP}{dz}.$$

The factor 4π has been inserted throughout the text; also N, the number electrons per unit volume. In the text K is the dielectric coefficient of the ether when devoid of matter.]

For the motion of the matter, if m be the mass of whatever moves with the electron, matter or effective inertia of ether, and H be the component of the impressed magnetic force normal to the wave, and k the coefficient of restitution of the matter-displacement which controls its free period,

$$m\ddot{x} + kx = e\mathrm{P} + e\mathrm{H}\dot{y}, \qquad m\ddot{y} + ky = e\mathrm{Q} - e\mathrm{H}\dot{x}.$$

It is to be observed that, if we assume the motion periodic, these equations can be reduced to the form that Drude* and Leathem† have shown to lead to results that agree with the observations on the effects of magnetized media on the transmission and reflection of light.

If we substitute in these equations what obviously solves them, the equations of a right- or left-handed circularly polarized wave of frequency $n = p/2\pi$, and wave-length $\lambda = 2\pi/q$, and whose amplitude in x and y is a, and in P and Q is A,‡ we get

$$(\mathrm{K}\mathrm{A} + 4\pi\mathrm{N}ea)p^2 = \mathrm{A}q^2,$$

$$a(mp^2 \pm e\mathrm{H}p - k) + e\mathrm{A} = 0.$$

If we substitute mp_0^2 for k, where p_0 is a measure of the frequency of the free period corresponding to the forces kx, ky which have been assumed above to control the motion of the electron, we get for the velocity of propagation $p/q = \mathrm{V}$,

$$\mathrm{V}^{-2} = \mathrm{K} + 4\pi\mathrm{N}e\frac{a}{\mathrm{A}}, \quad \text{and} \quad \frac{a}{\mathrm{A}} = \frac{-e}{m(p^2 - p_0^2) \pm e\mathrm{H}p}.$$

These enable us to calculate the dispersion of the substance in terms of the difference of velocity of propagation of waves of oppositely circularly polarized light.

To get approximate formulæ in terms of quantities that can be observed, we have the refractive index $\mu = \mathrm{V}_0/\mathrm{V}$, where V_0 is the velocity *in vacuo*, and $p = 2\pi\mathrm{V}/\lambda$. Assuming that λ_0/λ

* *Wied. Ann.*, 1896. † *Phil. Trans.* A, 1897.

[‡ So that $x + \iota y = ae^{\iota(pt - \varphi)}$, $\mathrm{P} + \iota\mathrm{Q} = \mathrm{A}e^{\iota(pt - \varphi)}$.]

is small, *i.e.* that the dispersion is due to an absorption band far up in the ultra-violet, we get, writing ρ for e/m,*

$$\mu^2 = \mu_0^2 + \frac{e\rho\lambda_0^4}{4\pi^2\lambda^2} \pm \frac{e\rho^2\lambda_0^4}{8\pi^2 V\lambda} H.$$

The second term on the right-hand side of this equation gives the ordinary dispersion, while the third term gives the Faraday rotation. The first term μ_0 is given by

$$\mu_0^2 = KV_0^2 + \frac{e\rho\lambda_0^2}{4\pi^2},$$

and is, as Mr. Larmor has pointed out, composed of two parts, the first being essentially refraction and the second dispersion.

In the case of air, it is possible to compare this equation roughly with observation. The equation is of the form

$$\mu^2 = \mu_0^2 + \frac{a}{\lambda^2} \pm \frac{a}{\lambda} \frac{\rho H}{2\pi V}.$$

We may estimate a from the dispersion in air, and it is approximately $1{\cdot}8 \times 10^{-14}$.

[* The exact equation appears to be

$$\mu^2 - 1 = 4\pi N e\rho \frac{V_0^2}{p_0^2} \left(1 \pm \rho H \frac{p}{p_0^2} - \frac{p^2}{p_0^2}\right)^{-1},$$

where $\lambda = 2\pi V_0/\mu p$; giving approximately when λ_0/λ is small, and the square of the effect of H is neglected,

$$\mu^2 = 1 + 4\pi N e\rho \frac{V_0^2}{p_0^2} + 4\pi N e\rho \frac{V_0^2 \lambda_0^2}{p_0^2} \left(\frac{1}{\lambda^2} \pm \frac{\rho H}{2\pi V} \frac{1}{\lambda}\right).$$

Of this the term independent of H gives the ordinary refraction and dispersion, while the term involving H represents the Faraday rotation. The first two terms are what is denoted in the text by μ_0^2, giving the index for very long waves; this, however, arises from the statical refraction alone, the terms involving λ^{-1} being the dispersional part of μ^2 which depends on syntony in the molecule.

In the formula for air (*infra*), which is correct as it stands, the assumed single syntonic wave-length λ_0 is given by $a\lambda_0^{-2} = \mu_0^2 - 1$.

For a treatment based on generalized dynamics of a more complicated molecule, and for references to recent advances in this subject, cf. Larmor, "Æther and Matter," 1900, Appendix F.]

The equation* gives for the two absorption bands that exist in the magnetic field, instead of the single one $p = p_0$,

$$p_0^2 - p_1^2 = \rho p_1 \mathrm{H}, \qquad p_0^2 - p_2^2 = -\rho p_2 \mathrm{H},$$

therefore

$$p_2 - p_1 = \rho \mathrm{H}.$$

So that, if δp be the difference of frequency for unit magnetic force,

$$\delta p = \rho.$$

Hence, for the difference of refractive index of the two circularly polarized rays, we have

$$\mu \delta\mu = \frac{a}{\lambda} \frac{\rho \mathrm{H}}{2\pi \mathrm{V}} = \frac{a}{\lambda} \frac{\delta p}{2\pi \mathrm{V}} \mathrm{H}.$$

Assuming, what is certainly not accurately true, that ρ is the same for all lines, and taking that Mr. Preston's estimate for some lines of Zn applies to oxygen, namely, that 1 Angstrom unit change of wave-length is produced by a field of 20,000 C.G.S. units of magnetic force, we get

$$\delta\mu = 4 \times 10^{-14}.$$

The observed rotation in oxygen gives a value for this of $3{\cdot}5 \times 10^{-14}$, so that it is about equal to the calculated value.

The discrepancy may be due to the fact that ρ is not the same for all spectral lines, and we could not reasonably expect to get an accurate result by assuming that the Zeeman effect in zinc can be used in calculating the Faraday effect in oxygen. On the whole I think the calculation shows that what must be a *vera causa* for a Faraday effect is the whole cause of it. From the mere fact that rotatory polarization is approximately inversely as the square of the wave-length and consequently vanishes for long waves, it follows that it is essentially a dispersion phenomenon.

[* At the absorption bands μ is infinite, or V vanishes, so that A/a on the last page vanishes; and p_1, p_2 are the roots of the equation thus obtained.]

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CONVERSE OF THE ZEEMAN EFFECT

[From *Nature*, January 5, 1899.]

I HAVE not seen it noticed that a converse action to the Zeeman effect should exist. A radiating atom in a magnetic field gives out circularly-polarized light. A circularly-polarized beam of light should cause a directed rotation of the electrons, so that the absorbing gas should be magnetized and exhibit magnetic force. If all the molecules in a c.c. of gas were caused to rotate their electrons in the same direction, it would possess quite a considerable magnetic moment. It is very improbable that the action of a circularly-polarized beam of light would control the motions to such an extent as that; but it is quite possible that, if a circularly-polarized beam of sunlight were passed through a strongly-absorbing gas, it would magnetize it to an observable extent. The same effect would probably exist in any medium in which absorption was principally due to syntony and not mostly due to viscous actions. Hence I would expect some effect with absorbing substances like fuchsine. It is doubtful whether lampblack, iron, or other metals, have a sufficiently syntonous absorption to exhibit the effect.

My assistant, Mr. Thrift, is engaged in trying the experiment, but in the meanwhile I thought it might be of general interest to point out that such an effect should exist.

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EXPERIMENT TO ILLUSTRATE THE ZEEMAN EFFECT

[From *Nature*, March 30, 1899.]

AN interesting dynamical illustration of the Zeeman effect may be made by fixing a gyroscope so that its axis of rotation is the line of suspension of a pendulum bar so suspended as to be capable of vibrating in any plane. When the gyroscope is rotating, the plane of vibration of the pendulum rotates with a precessional motion; and when the pendulum is caused to vibrate in a circular path its rate of description of its orbit depends on its direction of rotation round its orbit. The analogy to the Zeeman effect would make the rotation of the gyroscope correspond to the imposed magnetic force, and the motion of the pendulum to that of the electrons. The explanation of the motion by the properties of a gyroscope is pretty obvious. It may be a matter for further consideration whether there are analogies between the length of the pendulum, and its precession when describing elliptic orbits, and the Zeeman effects; the ordinary elliptic precession corresponding to such a phenomenon as the double sodium line.

[From *Nature*, April 13, 1899.]

Prof. A. Gray has kindly called my attention to his Royal Institution lecture of April 29, 1898, in which, nearly a year ago, he pointed out the analogy between a pendulum with a gyrostat in its bob, and the molecule of a gas vibrating in a magnetic field, which I called attention to in my recent letter.

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ON A HYDRODYNAMICAL HYPOTHESIS AS TO ELECTRO-MAGNETIC ACTIONS

[From the *Scientific Proceedings of the Royal Dublin Society.* Read December 12, 1899.]

FOR many years I have been advocating the hypothesis that the ether is of the nature of a fluid full of vortical motion, and that electromagnetic actions are due to particular arrangements of this motion, which is, in general, irregular or at least undirected.

Some years ago, Lord Kelvin, at a meeting of the British Association, and subsequently in the *Phil. Mag.*, ser. v., 1887, vol. xxiv. p. 342, published an investigation as to a possible transference of laminar wave disturbance through a turbulent liquid. He was not satisfied with the investigation, principally, I believe, because it presupposed that there would be inappreciable diffusion of motion. I am, however, inclined to think that, if a liquid become turbulent by drawing out and twisting together long thin vortices, the rate of translation of the vortex cores would ultimately become very slow, and that the consequent diffusion of motion would become slower and slower as time went on, and the turbulence become more and more fine-grained. This result follows from the continual drawing out of the vortex filaments by their tangling with one another. In being thus drawn out, their energy of circulation is continually being increased; and as the total energy per unit volume of the fluid must remain constant, it follows that the energy of translation of vortex core would continually

diminish. Hence there seems to me every reason to believe that wave disturbance can be propagated within such a turbulent liquid.

Subsequently Lord Kelvin showed (*Royal Irish Acad. Proc.*, ser. III., 1889, vol. i. p. 340) how to calculate a stable, steady motion for a hollow vortex surrounded by a tore round which the liquid was circulating, and concluded that it would be possible to fill space with a series of approximately straight hollow vortex filaments which would be in stable steady motion. This seems to finally dispose of the objection that diffusion of vortices must take place, at least in the case of hollow vortices; and by making the vortex filaments sufficiently fine, the space might be filled with full vortices whose rate of diffusion could be made as slow as necessary to explain the ether, even if it could be shown that such an arrangement of full vortices would not be stable.

What I now desire to call attention to is a hypothesis as to the nature of the wave motion which can be transmitted by a system of vortex filaments.

A vortex filament can have a spiral wave superposed upon it. The irrotational motion in the neighbourhood of this screw will be essentially the same as the distribution of magnetic force near a similar spiral wire carrying an electric current. There will be, on the whole, a flow along the inside of the spiral, but the motion of the fluid is complex. It could, however, be defined by a vector, whose direction was parallel to the axis of the spiral, and whose magnitude was measured by the square root of the mean square of the additional energy per unit length of the moving fluid above that of the undisturbed vortex. If then a space were filled with spiral vortices, all parallel to a given line, and causing flow in the same direction, there would be an increase in the energy per unit volume which could be measured by the square of a vector, say E. There would be, on the whole, a flow of fluid along the axes of these spiral vortices. Now, consider the case of a single spiral vortex surrounded by other parallel straight ones. These latter would not stay straight. They would be bent by

the action of their spiral neighbour, and spiral waves would be set up along them. How can we describe this transference of spirality from one vortex to one of its neighbours? It depends upon two vectors—one the vector parallel to the axis of the spiral, and the other a vector perpendicular to the two vortices. The vector then defining the transference, being itself defined by two rectangular vectors, must be a vector perpendicular to both, *i. e.* must, in the case of a spiral vortex surrounded by others which it is setting in motion, be distributed in circles round the spiral vector. What will the magnitude of this new vector depend on, and how can we define it further? Its magnitude will depend on how fast the spirality is being lost by the original spiral. If we call this new circularly distributed vector H, and make its magnitude such that its square is equal to mean energy of this new motion, then, assuming wave propagation, we get, on account of the relation of direction between E and H, and of their velocities being small compared with the irregular motions already existing (so that we can assume them to be superposable linearly), that H must depend linearly on $\dot{E}$; thus

$$\dot{E} = AV\nabla H,$$

where A is a quantity, a velocity depending on the structure of the medium, *i. e.* depending on the nature of the turbulency in the undisturbed ether.

If we now can assume that, in the general case, the energy of the medium is the sum of its energies due to these two vectors, which, so far as they affect one another, are at right angles to one another, then we can write for the energy per unit volume of the ether

$$e = E^2 + H^2.$$

From this, and the principle of conservation of energy, we can conclude that

$$\dot{H} = -AV\nabla E;$$

and from these that

$$S\nabla\dot{E} = 0, \text{ and } S\nabla H = 0,$$

so that, if at any time $S\nabla E = 0$, and $S\nabla H = 0$, they will continue so.

Now, these are the fundamental equations of wave propagation in the ether, and it only remains to explain wherein electric charges consist upon this hypothesis.

If we consider a point on a spiral vortex, and suppose that the spirality is so arranged that on both sides the flow of fluid within the coils is away from this point, then the spirality on one side of the point must be a right-handed screw, and on the other side a left-handed screw. Now, a point of this kind would be unique in the vortex. It would, so far as the fluid outside the vortex coils was concerned, be a sort of source from which fluid was flowing in all directions. This flow would, at a short distance from the source, be extremely slow, and the action between such points with their vortex spirals would be almost entirely confined to the actions already described between the spirals and the accompanying flows within and near them. On this hypothesis these vortex spirals would be representatives of the Faraday lines of force.

The hypothesis here put forward very tentatively does not include any supposition as to the nature of matter, nor as to how the singular points that represent electric charges, or electrons, can be connected with matter. At the same time it goes some way towards showing that the hypothesis—that the ether is a turbulent liquid—has great possibilities underlying it.

In explanation of the above, it may be well to state clearly the assumptions underlying it. It is *assumed* that the spirality described is propagated unchanged as a wave. This is justified by pointing out that this spirality is essentially the laminar motion investigated by Lord Kelvin, because it involves a flow in the direction of the axis of the spiral, and such a flow cannot take place along the direction of a vortex filament without a spiral deformation of a filament. Lord Kelvin illustrated his theorem by reference to a system of vortex rings which would, however, diffuse among one another in a way that was contrary

to one of his fundamental assumptions: while I am citing, as an example of his theorem, the case of infinite approximately straight vortex filaments which he has shown might, certainly if empty, exist in steady motion in presence of one another.

It has further been assumed that, initially, $S\nabla E = 0$. This may be justified as follows:—If we assume that, initially, the singular points are points from which a large number of long spirals proceed in various directions, it is evident the same number must enter as leave any surface which does not surround one or more of these singular points, and that the excess of those entering above those leaving will be a measure of the number of their singular points within the surface. Calling this latter ρ, per unit volume, we get at once $S\nabla E = 4\pi\rho$, initially. Where $\rho = 0$, we have $S\nabla E = 0$; and as $S\nabla \dot{E} = 0$ at all these points, we see that, even though the original distribution in long spirals rearranges itself among the surrounding vortices, nevertheless $S\nabla E$ will continue to vanish at all these places where there are no singular points.

It may also be worth while calling attention to the method of analysis used in this note. I have taken a vector E, to represent the various very complex and undescribed movements accompanying the spirality described, and the vector H to represent the still less clearly described movements accompanying the propagation of E. This may seem, at first sight, an unsatisfactory method, but it is really quite in accord with our methods of investigation in other cases. Besides the obvious case of temperature and entropy, which measure properties of bodies whose dynamical character is only very vaguely known, I may take the example of pressure in a gas as very similar. The pressure of a gas was for generations dealt with and most usefully employed long before the structure of a gas was understood. This state of affairs is quite analogous to the condition in which Maxwell left the ether theory of electromagnetism. He postulated and discovered the properties of electric and magnetic force, without explaining them by any dynamical theory as to the structure of the ether. The analogies he put

forward were just as vague and unsatisfactory, but certainly not more so, than the gaseous theories that depended on explaining the elasticity of a gas by that of atmospheres of the hypothetical caloric. The kinetic theory of gases explained the pressure and other properties upon dynamical principles, but when first propounded, and even still, the actual distribution of motion amongst the molecules, atoms, and within the atoms of gases is unknown; but that does not detract from the value of the dynamical theory nor make us hesitate to use pressure as a function of the state of the gas, although we do not know exactly what that state is.

I give these examples of pressure, temperature, and entropy to show that there is nothing abstruse or contrary to precedent in my assumption of E and H as representing states which are not, in their entirety, described or analysed. I might have instanced directed quantities as examples, such as the stress in a wire subjected to longitudinal pull. This is a very complicated state of stress which can be represented by a single vector, although in no single instance have we any except the very vaguest conception of what the actual state of affairs is inside the wire subject to this stress.

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MAGNETIC ACTION ON MOVING ELECTRIC CHARGES.

[From the *Electrician*, July 14, 1899.]

FOR many years I have been endeavouring to persuade various generations of my students to undertake to make experiments on the mechanical forces acting on electrified bodies in a varying magnetic field. The small amount of the force to be expected, and the difficulty of suspending highly electrified bodies near conductors in such a way as to be freely movable, have hitherto deterred any of my students from undertaking the experiment. The existence of these forces is familiar to all students of Maxwell's theories, especially in the form put forward by Mr. Oliver Heaviside, who has so long insisted on the correspondence between electric and magnetic phenomena, and shown that the electric forces due to a changing magnetic displacement are the same as the magnetic forces due to a change in the electric displacement or an electric current. I hope from the short notice in the *Electrician* last week that Lord Kelvin has succeeded in experimentally demonstrating the existence of these forces. As regards the suggestion that the rotation of atoms by these forces is the cause of the Zeeman and allied phenomena, I considered that when the Zeeman effect was first published and came to the conclusion that, though some very minute effect might be due to an action of the kind, this cause was incompetent to produce so large an effect as Zeeman had observed. Of course, all this depended on a variety of assumptions as to the probable amount of the electric charge in an atom, as to its size and density, and so

forth, all of which are only very imperfectly known; so that Lord Kelvin may have, and probably has, much better reasons for thinking that this action may be the cause of the Zeeman and allied phenomena than I had for rejecting this hypothesis. At the same time, my recollection is that Mr. Larmor told me that he had considered the question from this point of view before Zeeman discovered the phenomenon, and came to the conclusion that the effect to be expected was too small to be observable.*

From some points of view, this magnetic induction effect on the rotation of an electric charge is much the same as Lorentz's explanation of the different rates of rotation of charges rotating in opposite directions round the lines of magnetic force. Lorentz's theory does not fully account for the *change* in rate of rotation when the magnetic force is applied, while the effect of changing magnetic induction on a charge rotating in a plane perpendicular to the lines of induction provides a cause for the change in the rate of rotation. Whether the cause is sufficient depends on a variety of assumptions which are not easy to verify, but it may be one of the reasons why the lines are so thin.

I would call attention to the connexion between this electric action of changing magnetic induction, and the modification of Weber's theory of diamagnetism, which is required to make this latter theory consistent with the supposition that all electric currents are due to the motions of electric charges on atoms. Mr. Larmor must, I am sure, have gone into this matter pretty fully in his recent Papers in the *Phil. Trans.*

The suggestion that the Zeeman and allied phenomena are due to a rotation of the atoms is essentially that advocated by M. Becquerel. It is not easy to see how, upon this supposition, the different amount of the effect upon different spectral lines is to be satisfactorily explained, nor how the varying masses of the different atoms seems to have so little effect upon this amount.

[* Taking the mass moved to be that of an ordinary electrolytic ion.]

The rate of rotation required comes out of the order 10^{12} per second, and one would at first anticipate that such a rapid rate of rotation would produce easily observable gyrostatic effects on the material. It appears, however, from a rough calculation, that even supposing that the rotation of the matter as a whole causes the atoms to rotate (by far the most probable hypothesis), the gyrostatic effects of magnetization would be quite small. More than a year ago, when M. Becquerel was advancing his hypothesis, I got some experiments made in Trinity College laboratory to see if any effects of this kind could be observed, but we could not observe any. The experiments were made by projecting vortex rings in a liquid between the poles of a powerful magnet, as it was thought that, perhaps, some distortion of the rings due to gyrostatic forces might occur. I have not Maxwell's Treatise by me here; in it are described a number of experiments to find out actions of this kind, but I am not sure that any of them were designed especially to discover a gyrostatic action due to magnetization. Further experiments are needed now that we know approximately, from Zeeman's experiment, what amount of gyrostatic action might be expected if the Zeeman effect is due to a rotation of the atoms.

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MAGNETISM AND MOLECULAR ROTATION

[From the *Electrician*, August 4, 1899.]

LORD KELVIN has kindly shown me a proof of a paper, which will be published on Tuesday next in the August *Phil. Mag.*, explaining in detail the way in which he thinks that the rotation of electrified parts of an atom, due to their being in a changing magnetic field, might explain the Faraday rotation of the plane of polarization of light by magnetized matter. He supposes something analogous to an internal electrified gyrostat, to be set in rotation by the growing magnetic field, and the rotation to continue, owing to inertia and absence of friction and radiation, until the rotation is destroyed by the opposite electric force when the magnetic field decreases. These gyrostats are supported by springs inside a spherical shell embedded in some sort of soft jelly. There can be no doubt that such a structure would cause a rotation of the plane of polarization of distortional waves transmitted through the jelly in the direction of the magnetic force, just of the kind observed by Faraday in magnetized matter. He further considers the effect of this rotation of the enclosed gyrostats, upon the waves that would be produced in the jelly by the vibration of these gyrostats inside their surrounding sheaths, and concludes that "... each bright line is not split into two sharp lines, but is broadened...." "No simplifying suppositions as to the character of the molecule, such as symmetry of forces and moments of inertia round the axis of the ring, can possibly give Zeeman's normal results of splitting of a bright line into two sharp lines...." He goes on

to say, "Hence, although in 1856 till quite lately I felt satisfied in knowing that it sufficed to explain Faraday's magneto-optic discovery, I now, in the light of Zeeman's recent discovery, discard my old tempting gyrostatic hypothesis. . . ." Though he discards it, as giving an explanation of the Zeeman effect, he thinks that the rotation of electrified atoms due to the increasing magnetic induction may be the cause of diamagnetism, as I pointed out in my recent letter. Under these circumstances, it may be of interest to point out a modification of Lord Kelvin's molecule by which the difficulty of producing thin lines is surmounted, while rotation of the plane of polarization is produced by the rotation of part of the molecule.

Imagine one of Lord Kelvin's spherical atoms immersed in a soft jelly. It consists of a thin spherical shell attached to the jelly. Inside this place a concentric spherical shell, separated from the first by a symmetrical system of perfectly smooth non-conducting springs, so that the inside sphere may be able to turn round freely in any direction inside the outer one. Inside the inner sphere is suspended centrally, by a symmetrical system of springs, a heavy particle. This system can, by the oscillation of the inside heavy particle, emit a vibration of a definite frequency, *i.e.* a thin line in the spectrum. Now, if the outer sphere be positively and the inner one negatively electrified, and the whole be subjected to a growing magnetic induction, the outer sphere will be given a twist in one direction and the inner in the opposite direction. The attachment of the outer sphere to the jelly will prevent it from taking up a continuous rotation, but the inner sphere will rotate round the diameter parallel to the magnetic induction, and, being supported by perfectly smooth springs, will continue rotating as long as the magnetic induction lasts. When the magnetic induction ceases this will produce a twist on the rotating electrified sphere which will stop its rotation. During its rotation the sphere will carry round with it the central heavy particle. If this latter be vibrating, its vibrations will be affected by the rotation, and if it be kept in vibration by a plane polarized wave

motion, traversing the jelly in the direction of the axis of rotation, this rotating molecule will react upon the jelly in such a way as to produce a rotation of the plane of polarization such as is required to illustrate the Faraday rotation.

Although this is not exactly a gyrostat arrangement such as Lord Kelvin was looking for, there is no essential difference between them; and it enables us to illustrate the Faraday rotation by a purely dynamical model when once the electromagnetic action causing rotation is granted, and thus seems to fulfil the same function as Lord Kelvin's gyrostatic models. It has, however, the advantage of giving thin lines when emitting waves produced by the vibrations of the interior heavy particle.

In Lord Kelvin's gyrostatic models, as described in the Paper already referred to, the rate of rotation of any gyrostat depends on the angle between its axis and the direction of magnetic induction; but in the model now described any inside sphere will, for a given value of the induction, always rotate at the same rate, and always round the line of magnetic induction, so that the vibrations emitted by a large collection of these molecules in the same magnetic field will be all alike, and not of all varieties within certain limits, as in Lord Kelvin's model. I have spoken to Lord Kelvin about this modification of his model, and he was desirous that I should publish it now in the *Electrician*.

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ON THE ENERGY PER CUBIC CENTIMETRE IN A TURBULENT LIQUID WHEN TRANSMITTING LAMINAR WAVES

[From the *Report of the British Association for the Advancement of Science.* Read September 15, 1899. See No. 53 *supra.*]

In the *Phil. Mag.*, volume xxiv. page 342, October 1887, Lord Kelvin has given equations for the transmission of laminar waves through a turbulent liquid. He expresses doubt as to the possibility of any turbulency being possible to which his investigation would apply, owing to the rapid diffusion of the motion; and he illustrates his paper by reference to a liquid in which separate vortex rings are arranged in a regular cubical order which would, as he says, be almost certainly subject to the diffusion of motion which would vitiate his investigation. A few years afterwards, however, Lord Kelvin published in the *Proceedings of the R. I. Academy*, 1889, vol. i. p. 340, a paper in which he described an arrangement of long, thin, empty, vortex filaments, which he considered would be stable, and not subject to the diffusion and mixing which would vitiate the application of his wave theorem to the first turbulent medium he suggested. I have this year, in the *R. D. S. Proceedings* (p. 51). published a paper calling attention to the way in which laminar waves might be propagated by this latter medium, and have suggested a way in which electrons might exist therein. The paper is only suggestive, and cannot claim to prove much. I now desire to call attention to the expressions that Lord Kelvin has given for the structure changes that take place

when the waves he describes are being propagated through the medium, and to show how to calculate a quantity which must be proportional to the energy per c.c. of this wave-motion. I must refer to the paper itself for an explanation of the notation, as it would make this note very long to give it here.

The equations from which Lord Kelvin deduces the possibility of the propagation of laminar motion through a turbulent liquid are two:

$$(1) \quad \frac{df(y,t)}{dt} = -\,xzav\frac{d(uv)}{dy},$$

$$(2) \quad \frac{d}{dt}xzav(uv) = -\frac{2}{9}R^2\frac{d}{dy}f(y,t).$$

In this, R^2 is the mean square of the velocity of the original turbulency of the liquid.

The comparison of this with Maxwell's equations is obvious, and $f(y,t)$ may be either magnetic or electric action, and $xzav(uv)$ will then be either electric or magnetic. It shortens matters to call $\frac{2}{9}R^2 = V^2$, $f(y,t) = P$, and $xzav(uv) = \gamma$; so that the equations are

$$\frac{dP}{dt} = -\frac{d\gamma}{dy}, \quad \text{and} \quad \frac{d\gamma}{dt} = -V^2\frac{dP}{dy}.$$

If now we take the quantity

$$P^2 + \frac{\gamma^2}{V^2} = 2\Sigma$$

and integrate it throughout space, and then determine its variations with time, we find

$$\frac{d}{dt}\iiint \Sigma\,dxdydz = \iiint\left(P\frac{dP}{dt} + \gamma/V^2\frac{d\gamma}{dt}\right)dxdydz$$

$$= \iiint\left(P\frac{dP}{dt} - \gamma\frac{dP}{dy}\right)dxdydz.$$

Integrating the second term under the integral by parts, and

omitting the superficial terms which may be at infinity, or wherever energy enters the space under consideration, we get

$$\frac{d}{dt}\iiint \Sigma dxdydz - \iiint P\left(\frac{dP}{dt} + \frac{d\gamma}{dy}\right)dxdydz = 0.$$

Hence we see that Σ, which is of the right dimensions, must be proportional to the energy per c.c. of the medium. This is the same as in Maxwell's theory, so that there seems very little more besides interpretation of symbols to make a turbulent liquid a satisfactory explanation of the structure of the ether.

I am myself satisfied, though I think Lord Kelvin is not, that the turbulency of a sufficiently fine-grained purely and irregularly turbulent liquid would ultimately become so slow in its diffusion from place to place, that Lord Kelvin's investigation would apply to it.

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THE APPLICATIONS OF SCIENCE: A LESSON FROM THE NINETEENTH CENTURY

[Inaugural Address as President of the Dublin Section of the Institution of Electrical Engineers, February 22, 1900. From *Proc. Inst. E. E.*, 1900.]

I FEEL very much honoured by having been placed in the position I now occupy and by having to deliver this opening address to the Dublin Branch of the Institution of Electrical Engineers. I believe that we are one of the first of the branches that has developed into the meeting stage of our existence, and may congratulate ourselves on having passed through our larval transformations safely and rapidly, and on our having been the first to emerge into an imago.

The action of the parent Institution in founding these local branches is worthy of our grateful commendation. We are left perfectly free to develop our own life untrammelled by any rules except such as we would ourselves have necessarily chosen to govern our actions. We have the great advantage of being a branch of a most distinguished Institution of wide-world reputation, and that without paying any extra subscription. I hope that we will add to the life and work of that Institution, and thereby promote both our own interests and the welfare of mankind. Papers and discussions here will be taken as delivered to the Institution of Electrical Engineers, and, if of sufficient merit, will be published in its Proceedings, thus securing to us a wide world publication, while at the same time ensuring that Ireland is credited with the work done. Why, for example, should the long-expected account of the pioneer electric railway

at the Giant's Causeway be sent to London to be read and discussed: it can now come here, to Ireland, where the work was done, we can discuss its lessons, and if we perform our duties well, the account of this great Irish work will be sent all over the world by our powerful parent Society.

It is one of the banes of scientific publication that there are so many different scientific publications. One must look here, there, and everywhere, for what has been published. We are not going to add to this most serious and growing evil. By the patriotism of the parent Institution we need not start a local publication in order to secure credit for Irish work, done here, read here, discussed here. We can get all this credit and secure the advantage of local interest, and at the same time avoid adding to a serious and growing evil. Thus this action of the Institution will be "... twice blessed, blessing him that gives and him that takes."

The history of electricity in the nineteenth century is far too large a subject for an occasion like the present one, but certain aspects of this history convey valuable lessons for the future, and may well engage our attention in this the last year of the century, and may help us to lay the foundations for further advance in the next. The aspect of the history of electricity during the nineteenth century to which I desire to direct your attention is, as an object lesson of how to apply science to further the well-being of mankind. The history of any applied science might be considered in this aspect, but the history of applied electricity is particularly appropriate for being thus considered, for several reasons. The history is condensed within a few years; the discoveries of science have followed one another with extraordinary rapidity, and within a few years after the discoveries were made they have been applied to the use of man. It is just a hundred years since Volta discovered how to make continuous electric currents. Within a few years of that discovery their chemical actions were discovered and electric lights produced, both arc and incandescent. Twenty years afterwards the magnetic effect of an electric current was discovered by

Œrsted, its mathematical theory evolved by Ampère, and the law of its intensity worked out by Ohm. Some fifteen years afterwards Faraday discovered how to produce electric currents by magnetism. Immediately after the discovery of the principle of the conservation of energy it was applied to electromagnetism, and the foundation of our whole system of electromagnetic measurement was laid. Faraday's belief in the correlation of electricity and light was forged into a consistent theory by Clerk Maxwell, following lines suggested by Lord Kelvin, and this theory confirmed experimentally by Hertz. Such, in brief, is the scientific history of electromagnetism during the expiring century, and on this science practically all the applications of electricity depend.

I may pause for an instant to consider where this theory now lands us. The all-pervading ether has been realised as the means of transmitting light, electricity, and magnetism, and we are looking forward to its properties explaining chemical actions and gravitation. We are still looking for a theory of its structure which will give a dynamical explanation of its properties. We know how to express these properties by quantities we call electric and magnetic force, whose laws we know, but whose laws we are, as yet, unable to explain by any structure working on dynamical principles. So far as we know, the properties of electric and magnetic force are explicable upon dynamical principles; so far there is no known necessity for seeking for adynamical properties in the ether; so far we may hope to explain electromagnetism upon the dynamical principles of Newton's laws without invoking any other principles than those of force and inertia, as expounded in these laws. Until, however, a satisfactory theory of the nature of the ether has been actually invented, there will remain some doubt as to the adequacy of these fundamental dynamical laws to explain all its properties. The direction in which it is most probable that an explanation will be found is in the hypothesis that the ether is of the nature of a perfect liquid full of the most energetic motion. We know that a gas consists of separate molecules in

intensely energetic irregular motion. I expect that the ether is a perfect liquid in intensely energetic irregular motion: much more rapid than that of any gas: with a rapidity of internal motion comparable with the speed of light: maybe with enough energy in each cubic centimetre to keep hundreds of horse-power going for a year, if only we could get at it. So far as this hypothesis has been worked at there seems nothing impossible about it, but, on the contrary, much possibility in it, and, to my mind, its inherent simplicity confers on it a great probability.

Be that as it may, we now know that in the electric lighting of our cities, in electric tramways and railways, in electric furnaces and electrolytic vats, and in the other innumerable applications of electricity, we are harnessing the all-pervading ether to the chariot of human progress, and using the thunderbolt of Jove to advance the material welfare of mankind.

Having thus shortly considered the progress of electrical science, the history of the *applications* of electricity may be now summarised. Shortly after Œrsted discovered the magnetic effect of an electric current, this discovery was applied to telegraphy; and Faraday's discovery of how to generate electric currents by magnetism was almost immediately applied to the same use. Telegraphy developed rapidly, and many subsequent discoveries were due to the observations made in the practical application of electricity to telegraphy. This has been developing ever since, accumulating knowledge and applying the accumulations to produce more knowledge and more applications, till all this has resulted in the perfection of the multiplex telegraph and the wonders of the telephone and wireless telegraphy. No other department of applied electricity has had such a continuous development, hardly any interval elapsing between discovery and application in its case, while in almost every other case years have elapsed between discoveries and their application. It is especially the object of this address to call attention to the cause of this, and to the lessons to be learnt from it.

Within the first decade of the century electrolysis and the electric light were discovered; but, except on a small scale in electro-plating, it was reserved for the last quarter of the century to see their application to the general use of mankind. Before Her Majesty began to reign, Faraday had discovered how to generate electric currents by magnetic actions; but, except to generate currents to light a couple of lighthouses, no applications of Faraday's discovery to generate electric currents on a large scale was made till Wilde, Gramme, and Siemens worked at it, more than thirty years after its discovery. The application of electric currents to transmit power on a small scale was made in the electric telegraph years before any applications were made on a large scale. Except for a few experiments by Jacobi and others, the transmission of power by electric currents on a large scale is the work of the last twenty—one might almost say of the last ten—years.

Consider now what are the characteristics of the applications which developed continuously, and what were those of the applications which lay dormant for years. Maybe we can learn from this consideration how to arrange that, in the future, our discoveries may not lie for years dormant.

The most noticeable difference between the applications of electricity that developed and those that lay dormant is that those that developed were useful on a small scale, while those that lay dormant were not useful until developed on a large scale. Electro-plating and telegraphy were useful on quite a small scale. Experiments as to their efficiency could be conducted on the laboratory scale with quite cheap apparatus, and thus they were actually developed.

A recognised authority, who is fond of poking paradoxical fun at Professors, has recently stated that "the progress of telegraphy and telephony owes nothing to the abstract scientific man." I do not know exactly what he means by the abstract scientific man, but I do know that telegraphy owes a great deal to Euclid and other pure geometers, to the Greek and Arabian mathematicians who invented our scale of numeration and

algebra, to Galileo and Newton who founded dynamics, to Newton and Leibnitz who invented the calculus, to Volta who discovered the galvanic cell, to Œrsted who discovered the magnetic action of currents, to Ampère who found out the laws of their action, to Ohm who discovered the law of the resistance of wires, to Wheatstone, to Faraday, to Lord Kelvin, to Clerk Maxwell, to Hertz. Without the discoveries, inventions, and theories of these abstract scientific men telegraphy, as it now is, would be impossible. Maybe the paradoxer means by an abstract scientific man one whose work has not yet been used in telegraphy: in that case the statement is true, but then it is a platitude. Perhaps he does not consider that the work of these men conduced to the *progress* of telegraphy; but then he should maintain that Œrsted's discovery of the magnetic action of an electric current was no advance on the previous method of telegraphy by irritating snails. I daresay he modifies Sam Weller's idea and considers that algebra, geometry, mechanics, sound, heat, light, electromagnetism, chemistry, come by nature, but that we must learn the "Morse code."

He goes on, however, to say that "the fundamental principles and natural facts that underlie the practice of electrical engineering are the teachings of actual experience, and not the results of laboratory research or professorial teaching." I pass over the implication that laboratory research is not actual experience, and that professorial teaching is not founded on actual experience. I see that he must be in great difficulties in drawing the distinction he implies between experience in and out of laboratories. The difficulty he is in is quite natural, because there is no such difference. It seems to me impossible to understand how anybody who has not a bee in his bonnet, and who knows anything of the history of telegraphy, can make the extraordinary statement I have quoted. One would have imagined, apart from this authoritative statement, that "the fundamental principles and natural facts" underlying the practice of electrical engineering were the pile of Volta, the electrolysis of Davy and Faraday, the magnetic force of Œrsted,

the laws of Ampère, Faraday's magnetic induction, the conservation of energy of Joule and Helmholtz, Kelvin's theory of cables, Ewing's hysteresis, Clerk Maxwell's theory of electromagnetism, Hertz's electromagnetic waves, Branly's coherer. Every one of these is essential to some part of modern electrical engineering, and every one of them was the result of mere "laboratory research" and "professorial teaching." The quoted statement is one of the most absurd paradoxes ever propounded.

I am delighted, however, to see that Sir William Preece emphatically states that "the engineer must be a scientific man." That really concedes all that has ever been in dispute between scientific men and engineers. Of course no "laboratory research or professorial teaching" can teach all that an engineer should know. How to deal with a drunken workman, or a strike, or a city corporation, or a board of directors,—these, for example, have not yet been reduced to such scientific principles that they can be made the subjects of profitable "laboratory research or professorial teaching." These and many other things can only be learnt by experience outside schools and universities, and it is one of the important problems of education to decide where school education should end and apprenticeship begin. I would, however, appeal to all engineers not to expect pupils coming from schools to have been taught to be immediately useful. No doubt this would save their employer a lot of trouble, but it would prove that the school had wasted the time of its pupils in teaching them what they should learn in the office.

We have, then, seen that electro-plating and telegraphy were capable of development on a small scale, and were consequently largely developed by laboratory research.

The development of dynamos from Faraday's discovery required expensive experiments, and to test their efficiency on a large scale required very expensive experiments indeed. It was not possible to conduct experiments, that would be of much practical use, on the small scale on which laboratory experiments have to be conducted on account of the miserable pittance that

is at the command of scientific laboratories. The only opportunity of conducting experiments on a large scale is when an inventor can control capital, as, for example, if he is himself in the position of an engineer to some wealthy body whose money he can employ on experiments. Jacobi and others spent a good deal of money, no doubt, on experiments in power distribution by electro-magnetic engines; but their expenditure, though quite considerable as compared with the usual run of laboratory experiments, was as nothing compared with the enormous sums spent by the pioneers of modern electromagnetic machinery on *their* experiments.

What we have found, then, is that development depended on whether or no people experimented energetically upon how to render each discovery of practical utility; where experimenting was energetic, development was rapid; where experimenting was not energetic, development was slow. We have further found that the energy of experimenting depended on the money available; where little money was required development was rapid, but it was slow where large sums of money were required in order to perform valuable experiments.

We may further inquire how it happened that money and time became available for costly experiments. Money is available for laboratory experiments by the beneficence of private and public endowment, and time is available by the devotion of scientific men to the advancement of natural knowledge. These have been available because some few men have had faith in the desirability of knowledge, both for its own sake and for the material and moral advantage of mankind. Money has been available in England on a large scale in the past because of the enthusiastic faith of some very few men in the possibilities of scientific discoveries. One of the most remarkable instances of this faith was in the case of the great experiment of laying the Atlantic cable. A few men with strong faith impressed their belief on a few capitalists, and after years of most expensive experimental work they at last brought their great undertaking to a successful issue; the general body of capitalists meanwhile

looking on with amused incredulity. The development of the dynamo depended similarly upon the strong faith of individuals who spent immense sums of money and much time and energy on the subject, because they had faith in its possibilities. It is remarkable how many of the developments of scientific discoveries of the latter years of the century have been due to foreigners or firms with foreign leaders, such as Siemens Brothers. This has been largely due to the fact that foreigners are far in advance of us over here in their faith in the possibility of using scientific discoveries. The rapid advance of the applications of science in the last quarter of this century has been very largely due to the growth of this faith. It has grown to a strong conviction in the ordinary public of America and the Continent, and is growing daily stronger over here, but is still far weaker here than in other parts of the civilized world. The result of this has been that while the germs of many of the greatest inventions have been made within the British Isles, we have not been pioneers in any great advance in the applications of electricity since the development of submarine telegraphy. Possibly another cause has been our obstinate retention of our abominable series—one cannot call it system—of weights and measures. It is with great hopefulness that I see public opinion gradually growing in favour of the metric system. It is lamentable how the uninstructed prejudice of the man in the street influences progress. Here is a reform advocated by a large number of manufacturers and distributors, by chambers of commerce, and by almost everybody who has seriously studied the question, and yet it is delayed, not because the leaders of the State have any doubt as to its desirability, but merely because they do not think the public sufficiently intelligent for its introduction to be popular. Thus do we all suffer for the sins and ignorances of one another. It is a matter in which we in Ireland are particularly interested. I feel sure that under Continental guidance Ireland would long ago have adopted the reform, only that the obstinate conservatism of Britain has forced us to use its horrible series of weights and measures. If

only our Irish members would push forward this reform they would do some good by saving a year's school life for each child and facilitating computations in every department of life; it would be worth much more than the millions they say Britain is robbing us of. Owing to these amongst other things we have not been pioneers. No doubt there are some advantages in not being pioneers; we are saved the expense of preliminary experiments, and can often enjoy learning by the mistakes of others. On the other hand, we lose such industries as electric tramway construction and polyphase transmission of power, and can never expect to be in the forefront of progress; a decided misfortune when it comes to fighting against Creusot guns.

How does it happen that one of the foremost countries in advancing science has been one of the last to appreciate the possibilities of applied science? This has been due partly, no doubt, to our great success as manufacturers and as mere mechanical inventors. No doubt Watt was a truly scientific inventor, and even mere mechanical inventors are appliers of scientific knowledge that was discovered, in the most part, by scientific men centuries ago; but most of our success as manufacturers has been due to mechanical inventions and to our well-trained and expert artisans, and not to the useful application of recent scientific discoveries. This great success, and the absence of scientific training in our schools, and the want of contact between manufacturing and scientific society, have all contributed to prevent a due appreciation of the value of scientific discovery and experiment as a means of advancing the material wealth of society. The very word "science" has contributed to the mistake. As has been recently pointed out to me by Dr. Trouton, it would be impossible to say the same contemptuous things of "knowledge" as are said of "science." In Germany the word used, "Wissenschaft," is the one corresponding to our word "knowledge," and there nobody of any sense could say that "knowledge is all humbug," as is here often said, and still oftener thought, of "science."

How otherwise can one explain an eminent authority's

recent paradox, "Many people advocate the early teaching of science; I do not": and then he immediately goes on to advocate the early teaching of botany, zoology, and physiography. He seems to use the word "science" for what he calls "ill-digested text-book science, illustrated by experiments which generally fail." I don't suppose anybody advocates the teaching of this "science" even late in a boy's education. I am afraid he is trying to earn a cheap popularity with the uninstructed *laudator temporis acti* by sneering at the early teaching of science, and by an absurd misuse of the term that would not have misled his most ignorant hearer if he had used the word "knowledge."

We are, however, I confidently believe, already entered upon a more reasonable era. In England, even wireless telegraphy, the child of one of the most recent discoveries of science, has received sufficient recognition for very expensive experiments to be carried out with most valuable results, notwithstanding the cold shoulder given to the invention by Government departments and the fear it has inspired that it may be in the hands of the company promoter. Its history is most interesting as showing how much in an invention depends on the push and energy as well as upon the ingenuity of an inventor. In this connexion I would strongly endorse the complaint of the President of the Institution of Electrical Engineers in his opening address, namely, that capitalists are unwilling to advance money to try experiments on a sufficiently large scale to be of service in deciding their value on a very large scale. A company can be promoted with a capital of £100,000 for almost any conceivable object, but it is quite hard to get only £10,000 or £20,000 to try experiments. I look forward to the time when eager capitalists and energetic Government departments will importune inventors to be allowed to work out their discoveries, instead of requiring to be solicited and ballyragged by the inventor into an unwilling permission to try them. A few failures now and then, a few cases of being taken in by fraudulent or over-zealous inventors,

and our wavering faith in science collapses. How many failures are there in the iron trade! how many are taken in by specious company promoters! And yet the capitalist has faith in the "iron business" and in "limited liability companies." Would that our faith in science were equally strong! We might then hope that laboratories would be reasonably endowed with men and means to carry out experiments on a sufficient scale to be of use in testing their practicability, and improving the methods of new inventions. I am delighted to see that Sir William Preece expects laboratories to carry on all the researches engineers may require. That is quite as it should be, but such laboratories must be very highly endowed.

A physical laboratory that spends a couple of hundred a year on materials, instruments, and so forth, considers itself fairly well off. I wish I had anything approaching this to spend in Trinity College, Dublin. When will poor T.C.D. get credit for wanting to do more, very much more, than its very limited and precarious income permits? When can we expect the country or generous benefactors to learn that science on a large scale is at the basis of the material prosperity of the country, and that science on a large scale is very expensive. But what use is £200 a year in making experiments on a commercial scale? Ten thousand a year would be more like the figure required; and £10,000 a year could be most profitably spent on experimental work here in Ireland, on the one subject of utilising our bogs. It is most probable that the energy of their combustion could be transmitted to our towns to provide them with light and power; but the preliminary experiments are far beyond the capabilities of a scientific laboratory, and though success is very probable, it cannot be so certainly promised as to justify one in enlisting the assistance of capitalists who would consider themselves very badly treated if failure was ultimately discovered to be inevitable. If the capitalists were decently educated and could themselves appreciate the scientific situation, it might be allowable to enlist their assistance, as they

might be considered to be going into a speculation with their eyes open. In the present state of the education of capitalists in this country, they must depend entirely on the judgment of scientific experts, who cannot be expected to fully appreciate the business side of the subject. Then there are the questions of three-wire tramways, leaky telegraph lines, submarine relays, sun engines, of flying machines which Lord Rayleigh considers can be constructed if money enough were forthcoming, and of vacuum tubes as a means of illumination, and of numberless other matters already ripe for application, to say nothing of the innumerable scientific discoveries that have not yet been even suggested as having practical applications.

Besides these industrial laboratories, all our Government departments, such as the army and navy, should have large experimental organisations where any invention that promised success would be developed and seriously tried. The decision of what to try should not be left to mere officials, however distinguished, but should be referred to independent scientific advisers—persons who were not trammeled by official traditions, but were in touch with scientific advance and enthusiastic believers in it. If the country spent a couple of millions per annum on experimental work of this kind it would bear much fruit, and we should not find ourselves out-shot by semi-barbarous farmers.

Hope is the great incentive to exertion. Without it a nation is dead. Without it we lose all belief in the possibility of improvement, and improvement at once becomes impossible. The history of electrical engineering, the utilization of the all-pervading ether for the service of man, should strengthen our hope and our belief in the possibility of improvement. For has it not revolutionized society and enabled high and low, rich and poor, to lead better lives, by making life less hard and grimy, and thus improved the well-being of man both materially and, what is far more important, morally as well?

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ON THE SIZE AT WHICH HEAT MOVEMENTS ARE MANIFESTED IN MATTER

[From *Nature*, April 26, 1900.]

In the molecular theory of heat it is assumed that the motions of atoms and molecules are the motions upon which the phenomena of temperature depend. These motions are assumed to be very irregular, and the apparent uniformity of structure of a gas, for example, is attributed to the very small size and irregularity of the motions, which within any region of sensible size are the same, *on the average*, as within any neighbouring region. Within regions of molecular dimensions the distribution of motion is extremely irregular; neighbouring molecules are not in general moving at the same speed or possibly vibrating in the same way. Hence in this view the scales in which heat movements are manifested in matter are of molecular sizes, *i.e.* from 10^{-7} to 10^{-8} cm.

In addition, however, to all this matter, motion, and vibration, there are present ether motions of an irregular kind. Within any closed envelope at constant temperature the ether motions must be in statistical equilibrium with the motions in the envelope. The energy per c.c. of these ether motions will be considerable at high temperatures, and small at low ones. Many years ago I called attention to the energy per c.c. required in order to, in this sense, warm up ether, and showed that it was quite comparable with that required to warm up a rare gas. Now the size at which these motions are developed is comparable with the wave-length of the ether disturbances

of the period of the motion. In the case of irregular disturbances, such as cause temperature phenomena in solids and liquids, one cannot define precisely the size of the ether disturbances, because they are of all sorts of sizes, being irregular in the same way as the matter motions are irregular; but it is known that at each temperature there is a particular wave-length, round which the ether vibrations may be grouped, and that this average wave-length is shorter the higher the temperature. The wave-lengths of these vibrations, so far as they have been observed, vary from $2{\cdot}4 \times 10^{-6}$ to 10^{-6} cm.

It is at once evident that the average size of the ether motions is *very* much greater—quite a thousand times greater—than the size of the molecular motions. In the molecular motions we could not expect to find any irregularity of distribution within distances such as we can see with a microscope, because within a visible volume there there will be millions of molecules, and the average motion will be all that we can expect to see. Is it necessarily so as regards the ether motions which exist on so much a greater scale? Is there any way in which these very much larger-scale phenomena may be expected to affect matter on a scale comparable with their own size, and which consequently we might expect to be able to see, and which might produce effects on masses of matter consisting of millions of molecules?

We may consider in this connexion an analogy from sound. In sound we can have small solid objects, such as masses supported by springs, which give out waves in air very much longer than the sounding object. A tuning-fork, for example, may be only a few centimetres long, and may give out waves a metre long. The balance wheel in a watch vibrating quarter seconds would generate waves—feeble ones no doubt, but still waves—80 metres long, or some 8000 times the linear dimensions of the vibrating object. Similarly the vibrations of molecules generate ether waves many thousands of times as long as the linear dimensions of the molecules. On the other hand, solid bodies may be much longer than the air waves they

produce. A bar of steel vibrating longitudinally would be fifteen times as long as some of the air waves it would generate. A pipe full of air would be of about the same length as the air waves to which it would resonate,—not less than about a quarter as long. If, then, we had a large number of sounding bodies, some small ones like small tuning-forks, balance wheels, and such like, and others like pipes of a size comparable with the air waves, we would expect that when the small ones were all sounding, the larger ones would resonate to their corresponding waves, and thus be set in vibration by the waves originated in the smaller bodies.

In the case of electromagnetic waves we should expect the same result. If there are bodies comparable in size with the heat waves in the ether, which can have electromagnetic vibrations produced in them of the same periods as those emitted by the molecules, these bodies should resonate to these heat waves. Now, by utilising the ordinary process of conduction in metals, we know that it is possible for electromagnetic vibrations to exist in conductors of a small size, down to a few millimetres in diameter; and there is no reason to doubt that by means of conduction very much smaller bodies can have electromagnetic vibrations in them. Dr. Lodge, indeed, has suggested that the structures in the retina are of about the right size to resonate electromagnetically to waves of the frequency of the light waves that affect our eyes. Larger objects would resonate to the electromagnetic vibrations corresponding to the ordinary air temperatures. A sphere 10^{-3} cm. in diameter would, for instance, resonate to waves of about the greatest length that have been measured by Rubens and Nicholls, and a much larger one could have a harmonic of its fundamental tone excited in it by these waves. In addition to these vibrations in conductors, non-conductors of one specific inductive capacity immersed in a medium of a different specific inductive capacity could also have syntonic vibrations excited in them. From all this it seems quite certain that in small particles of matter there must exist, at all temperatures, electromagnetic vibrations of a

size comparable with the wave-lengths existing in the surrounding ether.

What sort of effects might we expect to be produced by these electromagnetic vibrations? Is there any prospect of our being able to detect them? What amount of energy may there be in this form of vibration on each particle? These are questions to which I am afraid I can only give very vague answers. To the first question, as to what effects may be expected to be produced by these vibrations, I can only suggest in the first place an unequal heating of the particle. The parts of the particle which are the electric nodes, where the electric current alternates and where there are no electric charges, these parts should be kept at a slightly higher temperature than the electric loops. If the particle were not perfectly symmetrical, this would lead to an unequal heating of the particle as a whole, and this may be a cause of those so-called Brownian motions of small particles immersed in a liquid which are so very difficult to explain. In the second place, it may lead to a grouping together of molecules into masses of a size depending on the temperature of the liquid, and to a going about of these groups of molecules and a similarity of the vibrations of the component molecules which complicates the theory of temperature in a way that may ultimately, as I have before now pointed out, explain to some extent the difficulties at present surrounding this theory. In the third place, this may be connected with the conditions for the breaking down of simple viscous motion and the production of vortices in a liquid, though I hardly think an explanation on these lines is required; and, finally, it may be connected with crystalline forces, the structures in the eye, vital actions in small cells and on a small scale, as in the patterns on diatoms, and possibly with the temperature at which vital actions of certain kinds, such, for example, as consciousness, are possible. These are the merest guesses of a wild kind as to the possible results of what seems to be a *vera causa* for structures and actions in matter of a size comparable with the wave-lengths of light, and must be taken as merely wild guesses.

As to the second question, of the prospect of detecting these electromagnetic vibrations in particles of matter, its answer depends so entirely upon the first that I can only leave it to the investigators of the future to try and detect them. That such electromagnetic vibrations exist, I think can hardly admit of doubt, any more than that the strings of a piano are kept in vibration when loud and irregular noises are produced in its neighbourhood.

As to the energy of the vibrations upon each particle, I cannot give any satisfactory answer. If the particle were in a region through which a series of plane waves of a constant type were being transmitted, it would no doubt be possible to solve the problem of determining the amplitude of its vibrations in particular cases of assumed shapes of particles. In the actual case of irregular disturbances I do not see, at present, any direct way of attacking the problem. It would apparently require to be attacked statistically; but I doubt whether this would lead to a true result, because there seems some reason to think that trains of uniform waves of considerable length do exist in the ether, and if there is any regularity of this kind in the ether motions, a purely statistical treatment, in which the vibrations were assumed to be quite irregular, would fail to lead to a true result. If the energy of these electromagnetic vibrations of its fundamental period on a particle is no greater than corresponds to its one degree of freedom on Boltzmann's theory of partition of energy, I am afraid the amount of energy of this kind on each particle is hardly sufficient to account for any observable phenomenon. That it may, however, be much greater seems justified by the failure of this theory, so far as is known, in other cases; and this must be my excuse for calling attention to what seems certainly a *vera causa* for structures and actions in matter of a size comparable with the heat vibrations in the ether, even though the amount of this cause may, when fully investigated, turn out to be so small as to be insufficient to produce observable effects.

97

THE RELATIONS BETWEEN ETHER AND MATTER*

[From *Nature*, July 19, 1900.]

THIS work is essentially the same as an essay to which an Adams Prize was awarded by the University of Cambridge. The subject for which the prize was offered was Aberration, and as this phenomenon, together with the Doppler effect on the frequency of light vibrations, are the only ones known due to the motion of matter through the ether (the spelling æther is disagreeably cumbrous), it naturally led to a discussion of the connexion between ether and matter, and the effect of their relative motion on the phenomena of electromagnetism.

There is a good deal of similarity between the development of this work and that of Maxwell's treatise. If one reads Maxwell's papers, it is pretty evident that he began with a somewhat definite hypothesis as to the nature of the strains in the ether to which he attributed electromagnetic phenomena; but in his treatise on electricity and magnetism there is hardly a trace of all this except in the reference to molecular vortices by which he justifies his equations for wave-propagation in magnetized media. In a similar way, Mr. Larmor has published papers in which the relations between ether and matter are developed in connexion with a suggestion as to the nature of an electron which is only hinted at in the body of the present work, and is relegated to an appendix with some deprecatory remarks as to its being merely an analogy to show that the

* *Æther and Matter*. By Joseph Larmor. Pp. xxviii + 365. (Cambridge University Press, 1900.)

properties of an electron are not impossible. The hypothetical structure attributed to an electron requires the medium to possess a very remarkable property which we do not find in matter, namely, an elastic reaction against absolute rotation of its elements; and although Mr. Larmor shows that gyrostats connected with these elements might confer such a property on them, he does not go so far as to develop any very definite structure for the medium, contenting himself with having shown that such properties as he assumes are not necessarily anti-dynamical. No structure for the medium that depends on gyrostats supported by a rigid framework can possibly be more than a rough working hypothesis, being, in fact, very little better than the brass wheel and india-rubber band, or tubes full of liquid with circulating pumps, that have been suggested as models to show that Maxwell's equations do not necessarily postulate impossible or adynamical properties for the ether.

But just as Maxwell's treatise is really independent of the dynamical analogies from which it grew, so Mr. Larmor's work is really independent of his suggested working analogy as to the structure of an electron. The whole work is based upon the hypothesis that electricity is atomic in its nature, there being only two kinds of atoms, positive and negative electrons. These electrons are, he supposes, essentially centres of strain in the ether, and move from place to place in much the same way as a drop of water might move through ice, melting in front and freezing up behind. Mr. Larmor leaves it for future investigation to determine whether there is any core, like that of a vortex ring, that accompanies this complex strain wave as it moves through the ether. As to the nature of matter, the only suggestion is that it consists of clusters of electrons in orbital motion round one another; but as the dynamics of such a system has never been worked out, it is impossible either to assert or deny the possibility of a permanent existence of such clusters. If this be the structure of matter, it certainly makes it probable that the transmutation of the elements is a possible development of chemistry, while a struc-

ture such as that of knotted vortices would make it improbable that we would ever be able to untie them and thus transmute one atom into another. There is the alternative possibility that we may find means of transmuting elements within any one of their related groups, but that we may find ourselves unable ever to transmute one group into another. Of course, if we ever found out some means of manufacturing electrons and matter we could probably transmute one kind of matter into another, though this latter might be possible according to Mr. Larmor's hypothesis, while the manufacture of either electrons or matter would be impossible.

All theories that explain electric currents by the motion of electrons are really based upon Rowland's classical experiment, that a moving electric charge produces the same magnetic effects as an electric current, and its converse that the electric force due to changing magnetic induction produces the same effect in moving an electric charge that the electric force due to another electric charge would produce, *i. e.* that electric force due to these two causes is the same. Our whole treatment of electromagnetism is practically based upon these same assumptions; but it is remarkable that so few attempts have been made to repeat this fundamental experiment of Rowland's, and no successful attempt seems to have been made to directly verify its converse. In a recent number of the *Comptes rendus* there is an account of a most interesting attempt to measure the electric current that one would expect to be produced in a surrounding coil when a convection current such as Rowland studied is being started and stopped. M. Crémieu has carried out an experiment on this with great care, and in a form in which one would certainly expect that the changing magnetic induction due to the magnetic force Rowland observed should produce an induced current in a coil of wire. He observed no such effect, and concludes that there is no magnetic force such as Rowland observed due to a moving electric charge. These moving electric charges are, however, in some respects, so imperfectly known that there may yet be some difference

between driving a current by mechanical and electrical forces, and that it is still possible there may be some other explanation than that drawn by M. Crémieu as the result of his interesting and important experiment. Mr. Larmor's investigations in this treatise of the effects of moving matter hardly touch the question raised by M. Crémieu, for his investigation is concerned with steady states, while M. Crémieu's experiment is essentially concerned with variable ones. If he is right and there is really no magnetic force due to a moving electric charge, and if consequently we must look to some other, possibly accidental, cause for Rowland's observation, it will certainly revolutionize the whole modern treatment of electromagnetism. The question raised by this experiment is, any way, one of the most fundamental ones in the connexion between ether and matter, and it is to be hoped that this question will be settled soon in a conclusive way, either by showing that M. Crémieu's conclusion is not justified by his observation, that his experiment really confirms a complete theory, or by overthrowing all our existing views, and leaving a free field for the twentieth century to build a new theory of electromagnetism on a firmer foundation.

In discussing the result of Michelson and Morley's experiments, from which they concluded that the ether is carried along by the Earth in its motion, Mr. Larmor shows that such a hypothesis is quite inconsistent with the fact of aberration and with the tenability of Sir George Stokes's suggestion that ether is like a very soft jelly. How such a soft material could be the means by which tramcars are driven by shearing stresses seems an additional difficulty in the way of this suggestion. Mr. Larmor concludes that the stone support on which the mirrors were borne changed in its dimensions, as it was rotated, by an amount proportional to the square of the ratio of its velocity to the velocity of light; and he justifies this by showing that if matter consists of clusters of electrons, just such a change of dimensions would take place as the experiment shows to take place. There is some difficulty in the hypothesis that the inertia of matter or any large part of it is like that of electrons,

and due to the motion of the neighbouring ether, because this involves the supposition that the inertia would change with the distance between the component electrons. That there may be some very minute effect of this kind is quite possible, though as yet undiscovered, but that any large effect of the kind exists seems extremely improbable. Possibly a careful study of the accuracy of Kepler's laws as applied to the solar system might show some discrepancy depending on a difference between the average distance of the electrons in such different materials as probably constitute Neptune and Mercury.

A previous question to all our explanations of phenomena by analytical dynamics is raised by Mr. Larmor in Appendix B, "On the scope of Mechanical Explanation; and on the Idea of Force." He has utilised the principle of least action throughout his work, and this appendix is a justification of his doing so, and besides raises questions as to the applicability of dynamical explanation to the growth and decay of vital organisms. Hertz objected to the adequacy of the principle of least action as a complete solution of all possible dynamical systems, because it is not generally applicable when rolling takes place, and we cannot be sure that rolling may not be one of the fundamental facts of the dynamics of the ether. Mr. Larmor dismisses this objection on the doubtful ground that "rolling is foreign to molecular dynamics." Hertz had also objected to the principle of least action for the semi-metaphysical reason that it makes the present state of the system depend on the future as well as on the past. As Mr. Larmor himself uses in his work the vector potential which makes the state at each place depend on what is simultaneously occurring at all parts of the universe, he naturally finds no objection to the principle of least action, because it makes the present depend on all future time. Neither of these methods is unobjectionable; each is an analytical juggle which has to be most carefully guarded lest it lead us into mistakes. The way in which the vector potential apparently locates the energy in the current instead of in the magnetic field outside it is a most serious objection to its use, although

Mr. Larmor seems to have steered clear of the difficulties raised by this curious complication. In a similar way the principle of least action is open to the objection of Hertz of making the present apparently depend on the future to an extent that does not apply to his own principle of the straightest path. It is a question for consideration in connexion with Mr. Larmor's discussion on the applicability of dynamics to vital phenomena whether the possibility of determining our actions by considerations as to the future is not connected with the possibility of analytically expressing the dynamics of the present by a formula which involves the future.

It will, from this meagre review, be evident that Mr. Larmor's treatise raises most fundamental and interesting questions, and is one that all who desire to strengthen the foundations of our knowledge of nature should carefully study.

98

ÆTHER AND MATTER*

[From the *Electrician*, July 20, 1900.]

ALL Nature, so far as physical investigation extends, consists of æther (why need one be bothered with that inconvenient diphthong? ether is much easier to write) and matter, and consequently Mr. Larmor's treatise naturally touches on an immense variety of fields. The basis of our theories of light, electricity and magnetism, chemical action, electrolysis, aberration, the Zeeman effect, are all touched on, together with a discussion on the Scope of Mechanical Explanation, and on the Idea of Force. It will thus be seen that it deals with questions which lie at the basis of our whole conception of the physical universe, and of our methods of explaining its phenomena. It would require more space than could reasonably be accorded to a review, and more time than the readers of the *Electrician* would mostly care to devote to the subject, to discuss in a way that could be followed the questions raised by Helmholtz, Hertz, and Larmor as to the sufficiency of the principle of least action as a basis of our mathematical analysis of physical phenomena, but the readers of the *Electrician* may naturally feel an interest in many of the questions dealt with in this important work.

In dealing with the phenomena of electromagnetism we may consider ether and matter as all one medium with different properties in its different parts, some parts being insulating and some conducting, some having one permeability and some

* *Æther and Matter*. By Joseph Larmor. (Cambridge University Press, 1900.)

another. In such a treatment it is not necessary to look for an explanation of these properties, and such a treatment covers a large number, indeed a *very* large number, of the phenomena. It is the method employed in our ordinary treatise on electromagnetism, and is quite sufficient for most purposes. But just as dealing with light as propagated in rays is a most useful method in optics, but breaks down when we study the subject more profoundly, so we are driven by many electro-chemical phenomena, by the electric discharges in vacuum tubes, and by a number of other phenomena in which the relation of molecules and atoms of matter to the ether is involved, to form some more definite hypothesis as to the connexion of ether and matter than the simple one that we are dealing with—a single continuous medium with different properties in different parts. Ever since Faraday discovered the law of electrolysis, that to the rupture of each chemical bond there corresponded the transference of the same quantity of electricity, no matter what the kind of bond might be, it has been in the scientific air that electricity as well as matter may be atomic. Maxwell, in this connexion, was driven to use the term 'atom of electricity,' although he deprecated it as quite contrary to the whole spirit of his treatise; but it is now recognised largely, thanks to Mr. Larmor, and to Prof. Lorentz of Leyden, that so far from this idea being contrary to the spirit of Maxwell's treatise it is in reality quite compatible with it, and from some points of view even essential. Mr. Larmor's book is mostly concerned with an elaboration of this view of the nature of electricity. He supposes that these atoms of electricity, or electrons, as Mr. Stoney called them, produce the phenomena of electric currents by their motion, and either move from molecule to molecule of matter, as in ordinary metallic conduction, or along with the atoms of matter, as in electrolytic conduction. To suppose that an electric current consists of opposite streams of positive and negative separate electrons instead of a continuous process taking place at all parts of a conducting wire necessitates a rather complicated study of the equations that are ordinarily used; and this is one of the

matters that Mr. Larmor devotes attention to, with the result that he finds that all the ordinary equations are applicable to the theory he is investigating, to the degree of accuracy attainable by our existing methods of studying the phenomena. It seems, however, pretty certain that if an electric current consists in the motion of separate electrons it cannot be *exactly* equivalent to a current uniformly distributed over the conductor. The self-induction of a current that runs in a set of separate wires is somewhat greater than that of a current uniformly distributed over the same area as that occupied by the wires, and one would consequently expect a small difference between the calculated and observed self-inductions of a coil, if this electron hypothesis be true. Hertz looked for but failed to detect anything of this kind. The effect to be expected is so very small that even he may very well have been unable to find it with the appliances at his disposal.

Mr. Larmor's fundamental treatment of the subject enables him to discuss effectively the changes in electric and magnetic phenomena that may be expected to arise owing to the motion of matter through the ether. In one of the appendices he enters into a discussion as to how this is possible when the ether is a medium that transmits transverse vibrations. The only other media we are familiar with that transmit transverse vibrations are solids, through which we cannot in general move without resistance. He shows conclusively that the suggested explanation of Sir George Stokes, that the ether is like a very soft solid, is untenable on account of the existence of aberration, and the absence of any evidence of slipping of the Earth through the ether with the consequent irregular distribution of relative velocity over its surface. His theory practically assumes that matter can move through the ether much in the way that a drop of liquid water can move through a lump of ice, namely, by melting in front and freezing up again behind. Upon this class of theory, matter and electrons are centres of some peculiar strain in the ether, and when they move it is this strain that moves like a wave through the ether, whether with a central

core, like the core of a vortex ring, or without one is not finally decided. The general conclusion at which Mr. Larmor arrives is that no terrestrial experiments yet devised can decide whether the ether is or is not stagnant near the Earth, because the phenomena would be exactly the same so far as our existing means of observation are concerned, whether the observer and all his surroundings were standing still relatively to the ether, or were moving with such velocities as 20 miles a second, which is comparable with that of the Earth in its orbit, and is the greatest at our disposal. In order to reconcile this with the result of Michelson and Morley's classical experiment he has to assume that the length of a body depends on whether it is moving lengthwise or sideways through the ether. To justify this he shows that, if the laws connecting the forces between material atoms vary with motion through the ether in the same sort of way as those between electric charges do, then just the required change of length that experiment demands would occur. This seems to make it practically certain that Michelson and Morley's experiment is really a very accurate measurement of the change of length of a body as it moves in different directions through the ether. As this theory would require that a diameter of the Earth should only change by about 10 cm. as it moves in different directions through the ether, the extraordinary delicacy of Michelson and Morley's experiment is made evident. It is to be regretted that no effect other than aberration and the change of frequency of light waves, due to the relative motion of the Earth and the ether, can be detected; for, if any considerable change in electromagnetic forces due to this cause existed, there might be some hope of our being able to utilise this relative motion to drive engines, and thus obtain a source of power practically unlimited in amount, and confined to no one part of the Earth.*

The book consists of fifteen chapters and six appendices. The fifteen chapters are substantially the same as the work to which an Adams prize was awarded to Mr. Larmor in 1898,

[* See Nos. 107–8 *infra*.]

and contain, in addition to the subjects already referred to, discussions as to the effect of the motion of the medium on the transmission of light through it, as to the nature of Röntgen radiations, and as to the pressure that light exerts on bodies that reflect or absorb it. The appendices contain an admirable summary of the relation of electrolytic conductivity to ionic theory and electric osmosis, and a short account of the theory that explains thermo-electricity by the motion of electrons. In Appendix E, Mr. Larmor develops a theory as to the intrinsic nature of an electron that is most interesting and suggestive, even if not in every respect unassailable. Advance, however, would be impossible if every hypothesis were relegated to the lumber heap that appeared open to serious objections. The wave theory of light was long discarded on account of the difficulty felt in reconciling it with the rectilinear propagation and the polarization of light, and even yet some difficulty attaches to it on account of the free motion of matter through a medium that transmits transverse waves. Yet the difficulty does not, fortunately, prevent our accepting the wave theory of light as even more than a working hypothesis. Mr. Larmor does not claim more than this for his suggestion as to the nature of an electron.

The last appendix contains an account of most of what is known as to the theory of the Zeeman effect and its connexion with Faraday's discovery of the effect of the magnetization of a medium in rotating the plane of polarization of light.

In all its parts the work is most interesting and suggestive, though in many places so condensed and general in its language as to be very difficult to follow. It marks a distinct advance in the scientific treatment of electromagnetic phenomena and important developments of our knowledge of the connexion between matter and ether.

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MR. HARGRAVE'S PAPER ON SAILING BIRDS

[From the *Aeronautical Journal*, July, 1900.]

I HARDLY think that Lord Rayleigh can have fully understood Mr. Hargrave's contention when he "fully agreed" with Mr. Maxim that the simple lifting up and down of the air by waves could not, without any onward wind, enable a bird to sustain its weight. It is of no consequence to a bird whether the air within which it is flying is or is not accompanied in its motion by the seawater or by neighbouring air, and if any simple upward draught of air can enable a bird to support its weight, the upward draught accompanying the back of a wave can do so. No doubt the bird must be falling relatively to the air in which it is moving, and from that point of view it might appear, at first sight, as if it must ultimately reach the water below it, which is rising at the same rate as the air. This, however, ignores the forward motion of the bird, which is constantly carrying it over a fresh water surface, which, though moving upwards with the same velocity as the last, began at a lower level, and is, consequently, when the bird reaches it, at the same level as the one it has just left, while this latter goes on and does reach, at the crest of the wave, a maybe higher level than the bird.

It is a mere question as to whether the bird, when moving through the air at the same rate as the waves, will, or will not, fall in still air at the rate of the upward draught on the face of a wave. If a bird, gliding downwards at a constant velocity in still air, goes forward at the rate of the wave and

sinks downwards more slowly than the air at the face of the wave is moving upwards, then it could maintain itself in this upward draught.

It might, at first sight, appear as if the question were the same as the problem as to whether a bird could fly over an undulating country when there was no wind, but the forward motion of the waves and consequent variable motion of the air over them makes the problem entirely different. If we could assume that there was no tangential force between the water and the air, and it must be quite small, the problem is essentially the same as that of the motion of an undulating country under still air, or what comes to the same thing the motion of air over an unmoving undulating country; and on every hand, by Lord Rayleigh and all other authorities I know of, it is conceded that a bird *can* use the upward draught of air blowing up a hill to sustain itself in a position fixed relatively to the hill. Now suppose the air to be stopped, and the bird and hill given a velocity equal and opposite to that of the air, and we have exactly Mr. Hargrave's case.

The real problem in the case of the bird is probably much more complicated because the motion of the air near the sea is not merely up and down, but is almost certainly, like that of the water, approximately in circles as the wave passes. The bird consequently has not only vertical, but alternating horizontal draughts to depend on, to enable it to sustain itself, so that I have no doubt Mr. Hargrave is quite right in his observation. The only case in which the bird would be unable to use the waves, would be when the wind was blowing at exactly the same rate as the waves were moving forward: that would be the same as if the bird were trying to fly over an undulating country when no wind was blowing. The case would not, however, be *exactly* the same, because there *is* a tangential action between the water and the air, which, in the case of the wind blowing at the same rate as the waves, would set up eddies, and prevent the lower air from moving as fast as the upper air, and might, in a variety of ways, make it quite possible for those

dexterous utilisers of differences of wind motion, the seabirds, to fly without exertion.

I feel the more interested in this question, because from the time of my first taking an interest in Lilienthal's experiments, I have looked forward to the possibility of human beings utilizing the trade winds blowing over the sea as means of locomotion. The irregularity of most winds renders their utilization by mankind almost hopeless, but the regularity of the trade winds and of the waves they produce makes their utilization quite possible.

100

THE THEORY OF IONS

[From *Nature*, September 27, 1900; being an abstract of a communication introducing a discussion on Ionization at the British Association.]

EVER since Faraday enunciated the law of electrolysis, that the same quantity of electricity passed when chemically equivalent masses of different substances were produced, it has been a matter of speculation whether this may not be due to atomic charges of electricity. Everyone, in describing electrolysis and explaining how the substances evolved appeared at the electrodes without any apparent action in between them, based his description and explanation upon the supposition of electric charges on the atoms. Some substances, such as hydrogen, were given positive, and some, such as chlorine, were given negative charges, and the electric current through the liquid was explained as due to the convection of these charges by the moving atoms or groups of atoms, and the movements of these were ascribed to the electric force acting on these charges. The amount of the charge on each atom or group of atoms was proportional to its valency, and as this has with good reason always been taken as a whole number, the charges ascribed to the moving elements were all simple multiples of the charge ascribed to a monovalent atom, such as hydrogen or chlorine. All this has naturally led to the hypothesis that electricity itself is atomic. In electrolysis, at least, there is a certain minimum quantity that corresponds to a single atomic bond, and quantities of electricity transferred by electrolysis are always multiples of this unit. It was surely natural, then, to give a name to this important physical unit

quantity of electricity, and it has consequently been called an "electron."

Further, in electrolysis, the electrons always appear connected with, and travelling with, certain atoms or groups of atoms. For example, in copper sulphate solutions, the positive electrons travel in pairs with the divalent copper atoms, and the negative electrons with the divalent atomic group SO_4. These charged atoms, or groups of atoms, playing such an important part in electrolysis, have been called "ions."

Now there is a very important difference between different liquids in their behaviour when we try to pass an electric current through them. Some are quite easily decomposed, others offer a very great resistance, and it has been a matter of most interesting speculation as to the cause of this. In the first place, most of the easily decomposed liquids are solutions in water, of acids, alkalis, or salts, and this has naturally attracted attention. In the second place, these solutions are all ones in which double decompositions and such-like chemical actions take place with facility. Can a common explanation be given of this remarkable coincidence of electric conductivity and chemical activity? Electric conductivity is due to two causes—first, the electric charges on the ions; and second, the independent mobility of these oppositely charged ions under electric force. Without entering upon the very interesting questions involved in innumerable speculations as to the causes of these charges and of the mobility of the ions, all modern theories acknowledge that, in some way or another, water, and some other liquids in a less degree, have the very remarkable property of conferring upon certain substances dissolved in them the wonderful independent mobility of the ions which we see in electrolysis. In consequence of this mobility of these differently electrified ions, it is easy to understand their chemical activity in these conducting solutions, and thus these two important properties of these solutions receive a common explanation. No really satisfactory explanation of how the solvent water confers on substances dissolved in it this wonderful independent mobility

of the ions has yet been proposed. Some writers have described the phenomena as if all that was needed was to assert that the ions move about independently, that the material $CuSO_4$, for example, is simply dissociated into Cu and SO_4, and that these ions gad about freely and independently in the liquid. Such writers consequently speak of the substance as being dissociated in solution. In what is recognized as ordinary chemical dissociation there is no different electrification of the constituents into which the substance is dissociated, and there is thus an essential distinction between the independent mobility of electrified ions in solution and what is recognized as chemical dissociation. Whatever be the true account of the matter, it is almost certainly very much more complicated than ordinary chemical dissociation, and the action of the water is evidently of the first importance. There is great difficulty in explaining this independent mobility, on account of two things which have not been satisfactorily explained as yet. In the first place, it is very hard to understand why these oppositely electrified ions do not combine together in pairs as they would do if they were merely under the action of the electric forces due to their opposite electrification. In the second place, it is very hard to understand where the energy comes from that is required to separate these independent ions and keep them free from one another. When copper sulphate is dissolved in water there is very little change of temperature; if it be the anhydrous salt, there is a rise of temperature; while we would naturally expect an enormous absorption of heat to account for all the energy that would be required to separate the Cu and SO_4 from one another.* This all shows how very different this phenomenon is from ordinary dissociation, and in consequence this peculiar action of water, and in a less degree of some other solvents, has been called "ionization." The two most important properties of an ionized fluid are conductivity for electricity, and a remarkably chemical activity which has been shown to be directly proportional to the conductivity.

[* See however p. 372 *supra*.]

Besides liquids there are gaseous conductors of electricity. Gases do not usually conduct electricity at all. Even under circumstances when one would naturally expect them to carry electric charges on their molecules, they seem quite incapable of doing so. When the surface of a liquid is electrified and it evaporates, one would naturally expect the escaping molecules of gas to carry away with them some of the electric charge from the surface of the liquid. It is not known whether an electrified metal volatilizing would or would not carry away with it some of the superficial electric charge,* but ordinary liquids volatilizing certainly do so to a very small extent, if at all. This may be, of course, because the charge is carried by superficial *ions*, and it is not the ions that escape, but the *molecules* of the liquid itself. But why these extremely movable ions cannot escape as a gas from the surface of the liquid is a matter still requiring explanation.

Under a great variety of circumstances, however, gases are able to conduct electricity. Leaving aside the spark, glow, and arc discharges in gases at high pressures, and the well-known discharges in gases at low pressures, in all which cases there is evidently something like a breaking up of the gas itself under intense electric force, there are a number of cases in which a gas can conduct electricity under quite feeble electric forces. Within any space at all close to a spark discharge of any kind, in flames and in the gases escaping from them, in the neighbourhood of surfaces of solids illuminated by ultra-violet light, in the neighbourhood of surfaces of solids being acted on chemically by the gas, in a gas traversed by kathode rays, and in a gas traversed by X-radiations and by those various, most curious and remarkable, radiations that have been classed under the name of Becquerel rays—in all these cases gases conduct electricity, apparently quite freely.

Under these circumstances it has been usual to describe a gas in this condition as "ionized," and to seek for a separate

[* A negative result for sodium vapour has been obtained by W. Craig Henderson, *Roy. Soc Proc.* 1900.]

and independent mobility of its ions. Great success has attended these investigations. The difficulties that surround ionization in liquids are mostly absent here. The ions, if left for a time to themselves, do combine together in pairs, and it requires a continuous and considerable expenditure of energy to keep them apart. The diffusion of the independent electricities has been studied, and many quantitative results that were to be expected from the theory have been proved to exist.

There is, however, an important difference between the conductivity of a gas and of a liquid. In the case of a liquid, the electricity always travels along with matter in the form of an atom or a group of atoms; in a gas there is every reason to believe that we are often dealing with electric charges which, if connected with matter at all, are connected with masses which are about 500 times smaller than a hydrogen atom. So far no good reason has been given for believing that the electric charges that move about among the molecules of a gas carry any matter along with them. There does not seem any difficulty in supposing that the electric charge of an atom can exist independently of the atom. All theories of electrolysis have supposed that electric charges are transferred within the liquid along with material atoms, but from the liquid to the electrodes all theories have supposed that the electric charges jump from the liquid atom to the electrode; and if it can jump from one atom to another, there seems no reasonable objection to believing in its independent existence. On account of the difference between the nature of the conductivity of a gas and of a liquid, it would be well to confine the term ionization to the case of conductivity due to the mobility of charged atoms or groups of atoms, and to call the conductivity due to the existence of these mobile electric charges which are not connected with atoms by another name, such as "electronization."

One of the most remarkable results of the study of these mobile electric charges which are unconnected with atoms is

that only negative electric charges have as yet been discovered to be free from atoms; the corresponding positive charges seem to be always attached to atoms or groups of atoms. This has naturally led to a rehabilitation of the old single-fluid theory of electricity in which matter plays the part of the positive fluid in the old double-fluid theories, and the phenomena of electronization, so far known, certainly lend support to this hypothesis. But we really know so little about the subject, that it is rather too soon to form anything beyond a rough working hypothesis. So long as we know that there exist outstanding second-order effects like gravitation, we are premature in concluding that the connexion between positive electrification and matter proves any first-order difference between positive and negative electricity, as it may be a second-order effect.

The conductivity of gases produced by the presence of these movable electric charges is in some respects more analogous to that of metals than to that of liquids. In liquids an electric current is accompanied by streams of matter, while in the electronized gas, so far as it has been observed, there may be no stream of matter attached to the electric charges which carry the current. It has consequently been suggested, and with good reason, that in solids and melted metals, which conduct metallically, the electrons are freely movable, and that this is the cause of their conductivity. There is some reason to believe that in this case also it is the negative electron which is most freely movable. Some most interesting calculations have been made upon this hypothesis, in which it has been supposed that there was something like a gaseous pressure of these mobile electrons in the metal. Thermo-electric effects have been attributed to the dependence of this pressure upon temperature, and the convection of heat accompanying electric currents has been attributed to the convection of energy of irregular motion by these electrons. The Hall effect has also been shown to be a possible consequence of a different mobility of the positive and negative electrons.

Upon these principles it is natural to attribute the magnetic properties of iron and other substances to electrons describing orbits round the atoms. These revolving electrons, in this case, represent the amperean atomic currents to which magnetization has long been attributed. A remarkable confirmation of this has been derived from the Zeeman effect, which can be explained by the supposition that negative electrons are describing orbits round the atoms. Further, the mass that moves with these electrons has been shown to be of the same order of magnitude as the 500th part of the mass of an atom of hydrogen, which, from experiments on gaseous electronization, seems to accompany the free electrons in a gas when it conducts.

There seems to be some reason to think that in a highly magnetizable material, such as iron, either there are more than the four electrons corresponding to its atomicity in rotation, or else that these are rotating very much more rapidly than corresponds to the vibrations of ordinary light. Some objection may be taken to the latter hypothesis from the difficulty of explaining why enormously rapid ether waves are not thereby generated in the surrounding medium, and the energy of the motion thereby lost by radiation. There are suggested explanations of this difficulty, but the other hypothesis, that matter has in it many more electrons than correspond to its atomicity, and that these latter are merely peculiar in being removable, agrees with a very interesting suggestion that all matter is built up of electrons. That an atom of hydrogen, for example, consists of some 500 electrons, one of oxygen of some 8000, and so forth. This is a natural deduction from these speculations, and receives some confirmation from its being consistent with the change in dimensions of a body as it moves in different directions through the ether which has been assumed in order to explain the experiment on the motion of the Earth through the ether, which Michelson and Morley conducted. A supposition such as this naturally suggests that atoms could be built up of electrons as well as the electrons separated from matter;

and if that be so, there seems no impossibility in the dreams of the alchemist, and an element of one kind may some day be transmuted into that of another. What is as yet known is, however, a very slender foundation for these speculations, and it is quite likely that matter and electricity are distinct in kind, and cannot be transmuted into one another in the way suggested.

Enough has been said in this very sketchy description of ionic theory to show how far-reaching it is; how it touches upon the confines of our knowledge and upon the borderland between physics and chemistry. Advances in our knowledge of ionic theory are likely to dispel many of the clouds surrounding the connexion of matter and ether, and may lay the foundations for an intelligible structure of the physical universe.

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MAGNETIC EFFECT OF ELECTRIC CONVECTION: NOTE ON M. CRÉMIEU'S EXPERIMENT

[From the *Report of the British Association for the Advancement of Science*, September 6th, 1900.]

M. CRÉMIEU has shown that, if his experimental methods can bear criticism as well as they seem to do, there is no induced electromotive force on a coil of wire surrounding a rotating disc when the strength of an electric charge on the disc is changing. He has deduced from this the conclusion that there is no magnetic induction through the disc due to the moving charge, such as Rowland's experiments showed. This note is to point out that too little is known of the theory of the ethereal effects of a charge of electricity forced to move by mechanical actions, for us to be quite sure that both M. Crémieu's and Rowland's observations may not be true—*i.e.* that it is possible that a charge of electricity, while it is being accelerated by moving matter, may produce such an action on the surrounding ether as to neutralise the electric force that would otherwise be produced by the changing magnetic induction due to the moving charge.

[From *Nature*, October 4, 1900.]

PROF. G. F. FITZ GERALD, in a note on Crémieu's experiment, described the arrangement adopted by M. Crémieu and the negative result he had obtained, and contrasted them with the arrangement and result obtained by Rowland in his experiments on electric convection made in 1876. He considered that the

discrepancy of the results of experiments, which appear to have been carried out with great care, did not necessarily disprove our theory of electromagnetism, but rather signified that there was some action of a moving ion not hitherto included in our equations which was well worth investigating. Dr. J. Larmor pointed out that any want of symmetry of the revolving disc and fixed case in Crémieu's apparatus would tend to cause some part of the charge on the disc to remain stationary. Prof. A. Gray announced that he had already commenced work with a view to repeating both Rowland's and Cremieu's experiments with the same apparatus.

102

THE NATURE OF IONS

[From *Nature*, October 4, 1900: report of a discussion at the British Association.]

PROF. G. F. FITZ GERALD opened a joint discussion with the chemical section, on ions. While acknowledging that the dissociation theory of electrolysis had proved a useful hypothesis, he wished to draw attention to the fact that there were phenomena which it was incapable of explaining, and that dissociation itself had not been dynamically explained. Why should water dissociate a dissolved salt into its ions? where does the necessary energy come from? how can the dissociated ions wander about in the solvent without recombining? and why do some ions travel faster than others? seemed to him questions which the supporters of the theory had never satisfactorily answered. The recent work on conduction in gases seemed to render it necessary to restrict the term "ionization" in future to the process of producing atoms differently electrified, and to introduce a new term "electronization" for the production of conductivity by the motion of particles of apparent mass about 1/500 of that of a hydrogen atom. In gases conductivity was probably due to both causes, and in liquids to the former only. In the case of metals, he should like to ask, how thick was the layer of electricity on the surface? did the thickness of a thin metal plate alter its capacity? and would the electrons fly to the surface of a metal when it revolved? He thought the questions still open to discussion might be summarized as follow:—

(1) The cause and nature of ionization

(2) The source of the energy in dissociation in a liquid

(3) The cause of the failure of the law of dissociation as the concentration increased

(4) The reason for the different rates of migration of the ions

(5) The nature of the double layers, or why different metals should attract electricity differently

(6) Are the processes of ionization the same in liquids and gases? and, if so, why?

(7) Do electrons gravitate, *i.e.* have they a material nucleus or not?

(8) Is magnetism due to rotation of the electrons?

Dr. Larmor, before calling on the chemists present for their remarks, pointed out that the large dielectric constant of water meant a large electric moment for the water molecule, and therefore a considerable separation of the positive and negative charges in the molecule. A molecule of dissolved salt might therefore readily come under the influence of one of these charges alone.

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SUN-SPOTS, MAGNETIC STORMS, COMETS' TAILS, ATMOSPHERIC ELECTRICITY, AND AURORÆ

[From the *Electrician*, December 14, 1900.]

IN reference to Principal Lodge's interesting letter, I would remark that at the time I suggested that the magnetic disturbances on the Earth might be due to electrified particles emitted by the Sun, the distinction between ions and free electrons had hardly claimed any attention.* What I suggested, as well as I recollect, was that the magnetic storms were due to something of the same kind as cathode rays, and I recollect that I made a calculation as to the density of the electrification—*i.e.* of the projected particles required in order to produce magnetic forces of the amount observed in magnetic storms, and that I was convinced that, with velocities such as had at the time been observed or calculated as existing in cathode rays, quite a feasible density of emanation from the Sun would suffice.

In connexion with this it may be worth while recalling attention to the way in which there seems to be some evidence from auroræ and magnetic storms that the Earth has a minute tail like that of a comet directed away from the Sun, the time of day of maximum magnetic disturbance being about 11 p.m.,

[* This was about the year 1893 (p. 298 *supra*), after Lord Kelvin had recalled attention in his Presidential Address to the Royal Society (*Roy. Soc. Proc.*, 1892) to the fact that terrestrial magnetic storms, of sensible amount, could not be due to direct magnetic action of the Sun. The theory formulated above has recently been re-stated and widely developed by Arrhenius.]

and this also being about the time of maximum auroral activity in each longitude. There are many things which seem to show that comets' tails, auroræ, the solar corona, and cathode rays are closely allied phenomena. Those who are interested in the solar effect on terrestrial magnetism should consult the elaborate study of these effects that has been made by Prof. Bigelow of Washington.

In connexion with the theory of comets' tails I would call attention to the difficulty of ascribing their repulsion by the Sun to the Maxwell pressure of radiations,* in that there is no evidence that the molecules of any gas absorb more than a *very* minute proportion of the radiations that fall upon them. For example, the area of the molecules in 1 cubic cm. of hydrogen is about 10,000 sq. cm., so that there are no gaps in a layer of hydrogen one ten-thousandth of a centimetre thick. Now we know that this thickness of any gas would absorb only a *very* small proportion of the Sun's radiation falling on it, and consequently we may safely conclude that the effect of solar radiation in reducing the gravitation to the Sun is quite insensible. The effect at all distances from the Sun is the same proportion of solar gravitation, so that if any effect of the kind were observable close to the Sun it would be equally observable as a correction on gravitation at all distances.

Dr. Lodge has called attention to M. Crémieu's experiments,† and fears that they may upset all the foundations of modern electrodynamic theory. I would suggest a possible way of reconciling these experiments with a great deal of our existing observations and theories. Suppose that when an atom moves through the ether with an attached electron, the moving atom is so affected that its motion disturbs the surrounding ether in such a way as to produce a magnetic force equal and opposite to that of the electron. This will entirely explain M. Crémieu's experiments, and all ordinary conduction due to the motion of electrons through matter and cathode rays due to free electrons. We require, however, a subsidiary hypothesis to explain the

[* See No. 21 *supra* and footnote.] [† See No. 101 *supra*.]

magnetic effects of electrolytic currents. This would take the form of a suggested difference between an ion and an atom. An ion on this hypothesis would so differ from an atom that its motion does *not* neutralize the magnetic effect of the electron. I acknowledge that there are most serious difficulties in the way of this hypothesis. It would certainly require us to suppose that the electric charges on the surface of a conductor are *not* ions. If they are so on the surface of a charged electrolyte, then Crémieu's experiments should produce magnetic force if he had used a moving liquid instead of a moving metallic conductor—a very difficult form in which to make the experiment. May be an electrified squirt of liquid, as from an Armstrong's electrical machine, could be used to see if there is any magnetic force near the squirting jet. Another serious difficulty is in deciding what happens at the terminals of an electrical machine, such as a Holtz or Wimshurst, when the conduction current with magnetic force turns into a convection current without. These difficulties are so great that, until M. Crémieu's experiments have been repeated by others,* and no doubt can be thrown on their methods or calculations, I would certainly advise at least a suspension of judgment before we seek for new foundations for our electromagnetism.

[* They have been in part repeated by H. Pender, of Baltimore, in the winter of 1900, with Rowland's apparatus and under his supervision, with results confirming the existing theory, *Phil. Mag.*, July, 1901 : also by others with like results.]

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OBSERVATION, MEASUREMENT, EXPERIMENT: SHORT ABSTRACT OF METHODS OF INDUCTION: MEASUREMENT OF TIME, MASS, LENGTH, AREA, VOLUME

[Hitherto unpublished: apparently the introduction to a projected treatise on physical measurement.]

ACCURATE measurement is of great importance in all branches of knowledge. Almost every science that has advanced much has done so owing to the discovery of accurate methods of measurement. Our knowledge of smells, tastes, hardness, and several other qualities of bodies are rudimentary, largely on account of the difficulty of finding means for measuring them at all accurately. Our knowledge of electrical science has advanced *pari passu* with the means of making electrical measurements.

The learning to make accurate measurement is essential for all who wish to be able to apply science to practice. Accurate measurement of the temperature of human bodies has resulted in great advances in the study of fevers. Accurate measurement of the strength of materials has resulted in great advances in our means of building bridges, ships, engines, etc. Hence those who are to spend their lives in applying science to relieve and prevent disease, or in directing the forces of nature to the use of man, should be specially trained not only in accurate observation but also in accurate measurement.

The habit of making accurate observations and measures is besides all this of very great value as an education. The habit of suspending judgment until the conclusion has been fully

tested, by varying the circumstances of the experiment and by repeated accurate measurement, is a valuable habit to acquire. The having to deal with facts and laws which cannot be evaded or cheated or changed, in accordance with predilections or prejudices or authority, is a valuable training in habits of truthfulness and accuracy. The cultivation and training of the practical ability to do things and to learn from observation, experiment, and measurement, is a part of education which the clergyman and the lawyer can maybe neglect, because they have to deal with emotions and words, but which the doctor and the engineer can only neglect at their own peril and that of those who employ them. These habits should be carefully cultivated from the earliest years while a child's character is being developed. As the twig is bent so the tree inclines. The fact that, in Ireland, these matters are not yet considered of any serious importance, and that the majority of medical and engineering students have had little or no training in observation, experiment, and measurement, is the more reason for attributing special importance to that part of their professional education which is concerned with Experimental Science.

On account of the extraordinarily perfect development of the sense of sight the observation of length is one that has been carried to the greatest accuracy. Indirectly we use observations of length to measure time and weight. We commonly measure time by the distance the hand on a watch moves; and we measure weight by the position of a balance. We can and do use our sense of hearing to estimate intervals of time, and we often use our sense of effort to compare weights, and we use our sense of taste to estimate how much sugar has been put in our tea; but none of these estimates can generally be made with the same degree of accuracy as estimates of length, or position, made with the sense of sight. Although this is generally true, nevertheless some of the most accurate methods of measuring small intervals of time depend indirectly on our sense of hearing, and some of the most accurate methods of

measuring length depend indirectly on our sense of touch. In general, however, when we want to invent some accurate method of measurement we endeavour to reduce it to an observation of the position of something, as for instance, in the case of time, to the position of the hand on a watch, and of weight to the position of a balance.

In measuring time, besides the hand on the watch, we require to be sure that the hand moves uniformly, that in going from I to II it always takes the same time to do so; and when we talk of a duration as three hours we generally require that other people shall understand the same duration as we do by the words. Hence there is in this, as in all other measurements, a necessity for some common standard that shall be understood and reproducible by everybody. Thus three hours means a duration of three times the duration of one hour, three yards three times the length of one yard, three pounds three times the weight of one pound. In each of these cases the numerical multiple is the same, but the quantity under consideration is different; and in every case of describing the amount of anything that can be measured numerically there are these two parts, the numerical part and that describing the kind of standard unit that is being used. Hence, in a physical question, one of the matters to be decided is always that of the standard units in which the quantities considered are to be measured. Without standard units of this kind, we cannot conveniently apply the laws of numbers to measure or compare the quantities considered, and are thus hampered in the use of an immensely powerful instrument of research. It is largely because no standard unit of smell that can be used as a numerical measure has been invented, that the science of smell is in such a very backward state. It has been largely due to the invention of standard units of heat measurement that the science of heat has made such great advances during the present century. In the measurement of time the standard unit employed is one that is employed in all civilized countries, and is the average time that the Sun takes to go from due south one day till it comes due

south on the next day; and this length of time is called a mean Solar day. The average time has to be taken because the actual time between the due southing of the Sun on two consecutive days varies from day to day throughout the year. We might of course use the length of some particular day, but the length of any one day could not be determined nearly so accurately as the average of a large number of days. Now when we say that the time the Sun takes to go from due south to due south varies from day to day it is evident that we must really be referring the matter to some more ultimate standard of uniformity of time than the motion of the Sun. We cannot directly compare one day with the next to find out which is longer; we must be really assuming some standard uniformity that each is compared with. We might compare them by means of clocks which we might reasonably assume on account of the uniformity of conditions to go at a uniform rate; and if we did so we would find that according to good clocks one day was not the same length as another day. As a matter of fact, however, clocks are not the standard by which days are measured. The standard with which the length of a day is compared is the time any fixed star takes to go from due south till it is due south on the next night. This is the time that the Earth takes to rotate on its axis, and there is every reason for assuming that the Earth rotates on its axis much more uniformly than any clock which we can construct. There has, however, been observed a very small change in the rate of rotation of the Earth. The day is a very little longer now than it was some thousands of years ago. This has been discovered by calculating very carefully at exactly what time of day certain eclipses that have been recorded should have occurred; and it is found that they actually occurred several hours earlier than the calculated time, showing that the Earth has made more revolutions since then than we calculated it would have made, and we attribute this to the rotation having been more rapid in former times than it is at present. There is good ground for thinking that the rate of rotation of the Earth is slowing down. The Sun and Moon make tides in the

ocean, and these tides are pulled back by the Sun and Moon while the Earth rotates, and there is thereby a very small frictional resistance produced, that tends slowly to stop the Earth's rotation, so that it is going a little more slowly now than it was thousands of years ago. But, it may be asked, how on earth can we discover a change in our standard itself? there must be a still more ultimate standard to which we refer it. If the whole universe, for instance, were to shrink to half its size and everything were to shrink simultaneously, we could never find it out by comparing lengths. All our yard measures would still agree, and all our foot rules would still be a third of the length of our yards, and every person who was five feet eight inches high would still be so according to the measures. We would require some more ultimate standard of length to compare our yards with, if we desired to find out a change of this kind. And there *is* a more ultimate standard of time postulated in the investigation of the rate of rotation of the Earth. When it was stated that the exact time of an eclipse was *calculated*, certain laws as to the motions of the Sun and Moon and Earth were postulated in those calculations. For instance the law of motion, that a body when acted on by no force moves uniformly, was postulated, just as this was also postulated when it was explained that it might be *expected* from the retarding effect of the tides that the Earth was now rotating more slowly than formerly. This expectation assumed that, apart from some cause of retardation, the Earth might be assumed to rotate uniformly. Another postulate, and an important one, assumed in the calculation is that the law of gravitation accurately represents the actions between the Sun and Earth and Moon and the other planets. Hence our real ultimate standard of time is an assumed accuracy of these laws, and there is a great deal to be said in favour of explicitly basing our measure of time upon these laws of motion and of gravitation. This is possible and has been more than once suggested. However, our actual standard of time is derived from astronomical observations of the stars; and astronomical observatories, such as those at Dunsink and Greenwich, are

engaged in continually making these observations, and keeping their standard clocks going uniformly by the standard of the stars. Other clocks, as for instance the standard clock in the hall of our Engineering School in Trinity College, Dublin, are kept going uniformly by an electric system of regulation in accordance with the standard clocks at the Observatories, and and thus all the clocks and watches in a country are regulated to keep time in accordance with the standard, the time of rotation of the Earth on its axis.

The average time then that the Sun takes to go from south to south is called a mean solar day. For convenience in measuring smaller intervals of time this is subdivided into 24 hours, and each hour into 60 minutes, and each minute into 60 seconds. This is a very ancient subdivision of the day, and is adopted in all civilized countries. It is not at all a convenient system of subdivision for facilitating numerical calculations. A subdivision of the day into 10 parts and each of these into 10 parts, and so on, would have been very much more convenient for numerical calculations; but there is no prospect that such a decimal division of time will be adopted, so that we must be content to put up with the use of hours, minutes, and seconds. At the same time, people who make a great deal of use of arithmetical calculations in respect of time very commonly do quote time either in seconds and their decimal multiples and submultiples, or in days or years and their decimal multiples and submultiples. This is practically the same thing as using a decimal division of time instead of dividing it into days, hours, minutes, and seconds.

The apparatus most commonly used in actual practice for the purpose of measuring intervals of time is some form of clock or watch. Clocks are commonly made with pendulums. The pendulum is a bar with a weight at one end, and hung from the other so as to be capable of vibrating freely. When the pendulum is long, it vibrates slowly; when short, it vibrates quickly. If, however, the point of support be not near one end, but near the centre of gravity of the pendulum, it may vibrate

very slowly indeed. This is the principle upon which metronomes are constructed. In them, by means of a movable weight, we can alter the position of the centre of gravity of the pendulum, and make it approach very close to the centre round which it oscillates, and thus make a short pendulum in a small space vibrate very slowly. This arrangement is not commonly used in clocks, as it is not easy to regulate it so as to vibrate accurately at the rate required in order that the hands of the clock may move at the right rate. The works of the clock consist of mechanism by which a weight or spring keeps the pendulum in vibration notwithstanding frictional resistances, and of a system of cog-wheels by which the hands conveniently count the number of swings of the pendulum. Thus, in a common form of large clock where the pendulum is of the length to beat seconds, the minute hand goes once round the dial when the pendulum has made 3600 vibrations, and the cog-wheels connecting the pendulum with the hand are designed so as to ensure this. Thus the hands of a clock are merely a counting arrangement for conveniently counting the number of beats of the pendulum. A clock with a pendulum which vibrates under the action of gravity cannot be carried about conveniently. It must be kept fixed in an upright position in order to go properly. Hence portable clocks and watches are not constructed with pendulums. In them a wheel, called a balance-wheel, is attached to a spring in such a way that, when the wheel is turned a little in one direction, it bends the spring one way, and the spring tends to bring the wheel back to its original position. When the wheel is turned a little in the other direction the spring again tends to restore it to its original position of equilibrium. When such a wheel is pivoted so as to turn very freely, if it is slightly turned round it vibrates, turning alternately one way and the other under the action of the spring. Such a wheel can be placed in any position, and acts equally well in all, and even pretty rapid changes of position produce but little effect on it. Hence its vibrations can be used in watches and clocks, and such timepieces will be portable. These timepieces must, of

course, be driven by springs because weights can only be used in fixed clocks, but the mechanism by which the balance-wheel is kept vibrating notwithstanding friction, and by which the hands count its vibrations is very much the same in all clocks and watches. The details of this mechanism are briefly described in an appendix, and students who feel an interest in it can study it there; and if they do so, and at the same time get an old clock, and take it to pieces, and make out for themselves how it is constructed and works, they will learn what should interest everybody, for everybody is dependent on clocks and watches for very useful information as to the lapse of time. When a pendulum or balance-wheel is disturbed, there is a force tending to restore it to its position of equilibrium, and the farther it is displaced the greater is this force. In balance-wheels this force is due to the spring which is bent, and in pendulums it is due to the Earth. The force exerted by a spring is almost exactly proportional to the amount it is bent, and the restoring force on a pendulum is nearly proportional to the amount of displacement when this is small. The consequence is that when the mass is much displaced, it is pulled by a large force, and so moves back quickly. If on one occasion it is displaced double the distance that it is on another, it will be pulled back with double the force; and when the subject is carefully investigated, it can be shown that it will move just so much faster that it describes the double distance in the same time as the single distance. Hence balance-wheels vibrate at the same rate whether they vibrate through a large arc or through a small one, and the same is approximately true of a pendulum so long as its arc of vibration is small. Different balance-wheels and pendulums vibrate at different rates. What is it that determines the rate at which a balance-wheel will vibrate? It all depends on how fast the spring can cause the mass that constitutes the wheel to move. If the spring is very weak and the mass large, the small force exerted by the spring will not be able to make the large mass move quickly in a short space, and so the balance will vibrate slowly. If the wheel is of large

diameter so that the greater part of its mass is a long way from the axle, the force exerted upon it by a weak spring when bent a given amount will be small and the distance it must move to return to its position of equilibrium will be large, so that it will vibrate slowly; while if the spring be very strong and the mass small and close to the axle the wheel will vibrate quickly. In the case of the pendulum the force tending to restore it to equilibrium will depend on the weight of the pendulum and on the distance its centre of gravity is from the axis. If this distance be great, there will be a very small restoring force for a given displacement, and so the pendulum will vibrate slowly. On the other hand, if the axis be very close to the centre of gravity, we must take account of the fact that a great deal of the mass to be moved may be a long way from the axis, and that although the restoring force at the centre of gravity may be considerable, yet that the effort exerted on each of the distant parts of the mass tending to move it round the axis will be very small; and thus we find that a pendulum vibrates slowly both when it is very long and when it is suspended from a point very close to its centre of gravity, and that there is a certain distance between the axis and the centre of gravity for which the time of vibration is less than for any greater or less distance. For a more complete investigation see appendix.... In every case the time of vibration can be calculated from the formula

$$T = 2\pi\sqrt{\frac{I}{m}}$$

where I is the moment of inertia of the mass round the axis, and m is the moment of the forces for unit displacement. In the case of a small mass, such as a bullet hung by a fine thread from a fixed point, this reduces to

$$T = 2\pi\sqrt{\frac{l}{g}}$$

where l is the distance of the bullet from the point, and g is the

acceleration of gravity. Such a small mass hung by a fine thread, in which we may assume with considerable accuracy that the pendulum moves exactly as if it its whole mass were concentrated at the centre of the bullet, is called a simple pendulum. Any pendulum or balance wheel will vibrate at the same time as an equivalent simple pendulum if

$$\frac{I}{m} = \frac{l}{g}, \quad \text{or} \quad l = \frac{Ig}{m},$$

which length is consequently often called the length of the equivalent simple pendulum.

It thus appears that clocks and watches are essentially machines for observing how fast a force can move a given mass. A pendulum clock consists of an apparatus for recording how often in a given time the force of gravity makes a mass move from the one end to the other of its swing; and a watch with a balance-wheel records how often the spring is able to make the balance-wheel vibrate from one end to the other of its swing.

By making the mass to be moved small and the strength of the spring great, we can cause a body to vibrate very quickly. Springs of stiff steel that have only to move themselves can be made to vibrate many thousands of times per second; and when studying the question of sound and vibrations, we will come across methods of recording their very rapid vibrations, and thus using them to measure very short intervals of time.

In determining the length of time any event takes to occur, there is often considerable difficulty in observing both the event and the hands of a watch at the same time. To get over this difficulty, stop-watches and other means of recording exactly an interval of time have been invented. Such an apparatus is often called a chronograph. Stop-watches are often made so that pressing a button causes the watch to start, and pressing it a second time causes the watch to stop. Thus we can easily measure the duration by starting the watch with our finger when we see the event begin and stopping it when the event ends. Then we read on the watch how many minutes and

seconds, and parts of a second, are recorded as having elapsed. With a little practice one can learn to do this to within about a fifth of a second of the true time. There is a certain delay in our operations between when we see an event and when we move our fingers, and this interval is not uniform unless we have practised performing the operation frequently under various conditions; and so the interval estimated may, even with a good deal of practice, differ from the real interval between the events by as much as a fifth of a second. This will be much more important in short intervals of time than in long ones. Thus if we are measuring the duration of an event that lasts three seconds, an error of a fifth of a second would be an error of one in fifteen or about 6 per cent. If we were measuring an interval of three minutes this same error of a fifth of a second would only be an error of one in nine hundred, or only about one-tenth per cent. In measuring long intervals of time with a stop-watch the want of accuracy in the going of the watch itself becomes of importance; while very short intervals of time cannot be accurately measured with a stop-watch, on account of this want of accuracy in response of our hands to our eyes. For measuring short intervals of time some mechanical method of recording the beginning and end of the event is essential. Some of these methods will be described when we come to study vibrations and sound.

One kind of short interval of time can be measured accurately by means of a stop-watch, or for the matter of that by an ordinary watch or clock; such is the time of swing of a pendulum, or the time of vibration of anything that vibrates sufficiently slowly for us to count the vibrations, or the time of rotation of anything that rotates sufficiently slowly for us to be able to count its rotations. If these vibrations and rotations are all of equal duration we can, very accurately indeed, measure the duration of each vibration. We need only find how long it takes for say a hundred vibrations or rotations to be performed, and then divide this interval by a hundred, and we thus obtain, very accurately, the duration of each vibration or rotation. In

making this determination we should observe a number of series each of a hundred vibrations or rotations, and take the mean, and the result will be more likely to be the true duration than any one of the observations. This process of making a number of observations with the conditions varied as much as possible, and then taking the mean, is one that should be followed by every careful observer. An isolated observation is seldom of very much value; it should be confirmed by subsequent observations. The circumstances should be varied as much as possible, and the observations and measurement made over and over again so as to check one another. Until this has been done one should not feel much confidence in observations or measurements. The chances of error are too great. The precautions against mistakes are too numerous for us to be sure that, in any one case, we have taken them all. But, if we have varied the conditions, if we have remade our observations and measurements, and we always obtain consistent results, then, and not till then, can we be perfectly confident in these results. This carefulness in observing and in measuring is a most important habit to learn. The want of it is one of the commonest causes of human error; and the habit is one of the surest safeguards against hasty conclusions and prejudiced judgments, the fruitful parents of ill-considered and inconsiderate actions.

. . . . . .

We must now arrange that the tube [of the observing telescope or microscope*] shall move parallel to itself and lie as nearly as possible at right angles to the measure, and that the measure itself be parallel to the object.

If five points on a body be constrained to move in five planes all parallel to the same lines, the body will be constrained to move without rotation parallel to this line. This is the geometrical principle employed in all such cases. Instrument makers are rather fond of endeavouring to make their instru-

[* This is the end of a section on measurement of length, which is not here printed.]

ments so that a great many more points shall be constrained; but as this requires very careful fitting and can never be accurately attained, it is really less efficient and much more likely to stick than if the above principle be attended to. In some cases the points that move on the planes must be carefully ground into small plane surfaces so as to distribute pressure and wear over a sufficiently large area, but in no case should the parts in contact be larger than is absolutely requisite. Instruments are often made with large plane surfaces in contact, but it is very difficult to prevent them from becoming either shaky or from jambing, and often both faults coexist. If we wish the body to be able to rotate round the line parallel to which it moves, four points may press against a circular cylinder (a shape which can be very accurately produced), and then the body will be able to rotate round the cylinder, and to move parallel to the cylinder, but will be able to move in no other way.

The way in which the condition for movement parallel to a fixed line is practically attained is to make a V groove with plane surfaces parallel to the direction in which the body is to move, the groove being cut in a plane surface also parallel to this direction. Any rounded peg going into the V will touch each of its surfaces in one point, and another peg would touch the surface in which it is cut, thus giving three out of the five points required to move on the five planes. The two other points moving on the other two planes may be a peg moving in a parallel V, or more conveniently another peg beyond the first one, and moving in the same V.* The three plane surfaces may, of course, be arranged otherwise, but this is one of the simplest cases. In order to keep the points from lifting off the planes it is often necessary to have a spring pressing against the back of the guide. Similarly in the case of the four points moving on a cylinder, it may be necessary to place a spring in such a position as to ensure that the points are not lifted bodily off the cylinder. It is by such trammels that microscopes are

* A little consideration will show that the body with the pegs on it can in this case only move parallel to the V groove.

constrained to move, without rotation, parallel to themselves and in a line parallel to the measure. We may sometimes wish to be able to read the position of the microscope to small fractions of the divisions on the measure, and then we may attach to it a vernier, or mount it on a micrometer screw, or use some of the other methods for reading subdivisions of a scale.

The setting of the tube of the microscope at right angles to the measure is often left to the instrument maker, but in some instruments, as for example in cathetometers used for measuring vertical heights accurately, where it is very important to have the line of collimation horizontal, special arrangements are introduced for setting it. It is always of great importance to have the line of collimation perpendicular to the measure when the marks to be observed are at different distances from the measure, and this often occurs in measuring differences of level. In this instrument two adjustments are essential, the measure must be vertical and the observing telescope must be horizontal.

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STRIÆ OF VACUUM TUBES APPLIED TO AURORÆ

[1893. Hitherto unpublished.]

LAW of densities is that width is *q. p.* inversely proportional to density. Law of diameter is that it is proportional to diameter; therefore to double the density is same as to halve diameter of tube (Pember, *Wied. Ann.* 1882, p. 279). In a tube 2 cm. wide striæ are a cm. apart; therefore, in one 10^5 cm. wide, striæ are 10^5 cm. wide.

Hence, if the atmospheric striæ of the aurora borealis subtend an angle of 20° from a height of 20 to 30 km., they might be moving [as if] in a tube 8 or 10 kms. wide, not at all an extravagant hypothesis. They move 20^c per sec., *i.e.* 10 km. per sec., or 10^6 cm. per second. Now velocity of sound $= 3 \times 10^4$ cm. per sec.; and at that temperature it would be

$$v = 3 \times 10^4 \sqrt{\frac{T_0}{273}};$$

say $\qquad T_0 = 100, \qquad v = 1{\cdot}7 \times 10^4$

not so very different; but it shows that their velocity unless they are very much nearer than one would expect, was many times as great as the velocity of sound, maybe 100 times as great.

Of course the velocity of propagation of such a phenomenon may not be a measure of a transference of anything from particle to particle. A stria is a place where the conduction of current is accompanied by a disorderly vibration, the dark space between striæ must conduct electricity just as well as the place where there are bright striæ. Mr. Spottiswoode's observations

with a revolving mirror (*R. S. Proc.*, vol. xxv., by some extraordinary oversight he omits to give any even approximate measure of the velocities he has described) show that the state of affairs in the interval between striæ is particularly favourable for the production of striæ by a current in the opposite direction, showing that there must be some polarized state analogous to Grotthus chains in these regions whose breaking up produces vibrations. Hence the velocity of motion of striæ, which is so much greater in large than in small tubes, is really only a measure of the rate at which these irregularities of motion are propagated; and as the tendency to arrangement is due to a force which is propagated with the velocity of light, there is no necessity that the irregularity should be propagated from particle to particle by collisions, for an action of that kind could not be propagated very much more rapidly than sound.

From Spottiswoode's observations, it would appear that the light-production in each stria begins at its negative side, and proceeds with a velocity comparable with the velocity of the striæ, in his tubes somewhat more rapidly, along the tube in an opposite direction to the current. These velocities are of course incomparably slower than that at which the electric actions are propagated. Many striæ show that, with the intermittance he was using, due to sparks and capacities connected with the tubes, the light production in the negative face of a stria began for the next rush of current, long before the positive face had ceased to cause light due to the former rush of current. These actions in an aurora seem to be somewhat more rapid than the velocity that Prof. J. J. Thomson ("Elec. & Mag.," p. *) has calculated for the cathode streams. These he has placed at five or six times the velocity of sound.

There seems to have been a double system of streamers in the recent aurora; one set diverging from the edge of the northern arch and another set nearly perpendicular to the surface of

[* The reference has not been found. His experimental determination of a much higher velocity, and very minute masses of the particles, came subsequently in 1897.]

the Earth. This would be accounted for by a very bright set of streamers concentrated along the edge of the northern arch, and a second fainter set perpendicular to the surface of the Earth. These latter streamers seem very much like cathode rays, streaming off perpendicular to the surface of the conductor; and Lenard's observation of cathode rays in air at ordinary pressures would seem at first sight to justify Lemström's disputed observation of auroral streamers quite close to the Earth, if it were not that in air at these high pressures there is no evidence of "streaming," the action being like that of a projectile amongst innumerable others that obstruct its path. It is interesting to know that Spottiswoode and Moulton ("Phil. Trans." 1879, p. 584) showed that cathode rays could be produced by violent sparks in gases at high pressures, and stated their opinion that there was nothing to prevent their being generated in gases at atmospheric pressure. They also showed that these rays behaved like a projectile moving amongst a lot of others, and that the rarer the air was the less the ray was impeded. The *illumination* seen is evidently dependent on this impeding of the ray, and the dying of it off in very perfect vacua does not by any means prove that matter is not the basis of the ray; it only shows that the ray is not impeded, just as the dark spaces between striæ are not failures in the electric circuit but are most probably places where the conduction by the matter is most perfect.

It is very hard to see what can cause such a slow sequence of the discharges as is observed in auroræ. A periodicity of a 10th to a 20th of a second or less could be easily accounted for, but one of once or twice a second is not so easy to understand. It seems as if it must have a *material* basis, be connected with the velocity of sound, and not of electricity.

106

ON THE RELATIONS OF RADIATION TO TEMPERATURE

By Dr. J. Larmor, F.R.S.

[From the *Report of the British Association*, Bradford, September, 1900. Reprinted in connexion with No. 23, and on account of footnote.]

The key to this subject is the principle, arrived at independently by Balfour Stewart and Kirchhoff about the year 1857, that the constitution and intensity of the steady radiation in an enclosure is determined by the temperature of the surrounding bodies, and involves no other element. It was pointed out by Stewart* that if the enclosure contains a radiating and absorbing body which is put in motion, the temperature being uniform throughout, then the constitutions of the radiation in front of it and behind it will differ on account of the Doppler effect, so that there will be a chance of gaining mechanical work in the restoration of a uniform state. There must thus be some kind of thermodynamic compensation, which might arise from æthereal friction, or from work required to produce the motion of the body against pressure excited by the surrounding radiation. The hypothesis of friction is now out of court in ultimate molecular physics; while the thermodynamic bearing of a pressure produced by radiation has been developed by Bartoli and Boltzmann (1884), and that of the Doppler effect by Wien (1893).

Application of the Doppler Principle.—The procedure of Wien amounts to isolating a region of radiation within a perfectly reflecting enclosure, and estimating the average

* *Brit. Assoc. Report*, 1871; cf. also *Encyc. Brit.*, art. 'Radiation' (1886), by Tait.

shortening of the constituent wave-lengths produced by a very slow shrinkage of its volume. The argument is, however, much simplified if the enclosure is taken to be spherical and to remain so; for it may then be easily shown that each individual undulation is shortened in the same ratio as is the radius of the enclosure, so that the undulatory content remains similar to itself, with uniformly shortened wave-lengths, whether it is uniformly distributed as regards direction or not, and whatever its constitution may be. But if there is a very small piece of a material radiator in the enclosure, the radiation initially inside will have been reduced by its radiating and absorbing action to that corresponding to its temperature. In that case the shrinkage must retain it always, at each stage of its transformation, in the constitution corresponding to some temperature. Otherwise differences of temperature would be effectively established between the various constituents of the radiation in the enclosure; these could be permanent in the absence of material bodies; but if the latter are present this would involve degradation of their energy, for which there is here no room, because, on the principles of Stewart and Kirchhoff, the state corresponding to given energy and volume and temperature is determinate. Thus we infer that if the wave-lengths of the steady radiation corresponding to any one temperature are all altered in the same ratio, we obtain a distribution which corresponds to some other temperature in every respect, absolute intensities [not] excepted.

*Direct Transformation of Mechanical Energy into Radiation.** —There is one point, however, that rewards examination. When

* The present form of this argument arose out of some remarks contributed by Professor Fitz Gerald, and by Mr. Alfred Walker of Bradford, to the discussion on this Paper. Mr. Walker points out that by reflecting the radiation from a hot body, situated at the centre of a wheel, by a ring of oblique vanes around its circumference, and then reversing its path by direct reflexion from a ring of fixed vanes outside the wheel, so as to return it into the source, its pressure may be (theoretically) utilized to drive the wheel, and in time to get up a high speed if there is no load: the thermodynamic compensation in this very interesting arrangement lies in the lowering of the temperature of the part of the incident radiation that is not thus utilized.

undulations of any kind are reflected from an advancing wall, there is slightly more energy in the reflected beam than there was in the incident beam, although its length is shorter on account of the Doppler effect. This requires that the undulations must oppose a resistance to the advancing wall, and that the mechanical work required to push on the wall is directly transformed into undulatory energy. In fact, let us consider the mechanism of the reflexion. Suppose the displacement in a directly incident wave-train, with velocity of propagation c, to be $\xi = a\cos(mx + mct)$; that in the reflected train will be $\xi' = a'\cos(m'x - m'ct)$, where a', m' are determined by the condition that the total displacement is annulled at the advancing reflector, because no disturbance penetrates beyond it; therefore when $x = vt$, where v is its velocity, $\xi + \xi' = 0$. Thus we must have $a' = -a$, and $m' = m\dfrac{c+v}{c-v}$, in agreement with the usual statement of the Doppler effect when v is small compared with c. Observe, in fact, that the direct and reflected wave-trains have a system of nodes which travel with velocity v, and that the moving reflector coincides with one of them. Now the velocities $d\xi/dt$ and $d\xi'/dt$ in these two trains are not equal. Their mean squares, on which the kinetic energy per unit length depends, are as m^2 to m'^2. The potential energies per unit length depend on the means of $(d\xi/dx)^2$ and $(d\xi'/dx)^2$, and are of course in the same ratio. Thus the energies per unit length in the direct and reflected trains are as m^2 to m'^2, while the lengths of the trains are as m' to m; hence their total energies are as m to m'; in other words the reflected train has received an accretion of energy equal to $m'/m - 1$ of the incident energy, which can only have come from mechanical work spent in pushing on the reflector with its velocity v. The opposing pressure is thus in numerical magnitude the fraction $\left(\dfrac{m'}{m} - 1\right)\dfrac{c}{v}$ of the density of the incident energy, which works out to be $\dfrac{c^2 - v^2}{c^2 + v^2}$ of the intensity of the total undulatory energy, direct and reflected, that is in front of the reflector.

When v is small compared with c, this agrees with Maxwell's law for the pressure of radiation. This case is also theoretically interesting, because in the application to æther-waves ξ is the displacement of the æther elements whose velocity $d\xi/dt$ represents the magnetic force; so that here we have an actual case in which this vector ξ, hitherto only introduced in the theoretical dynamics of electron-theory, is essential to a bare statement of the facts. Another remark here arises. It has been held that a beam of light is an irreversible agent, because the radiant pressure at the front of the beam has nothing to work against, and its work is therefore degraded. But suppose it had a reflector moving with its own velocity c to work against; our result shows that the pressure vanishes and no work is done. Thus that objection to the thermodynamic treatment of a single ray is not well founded.*

This generalisation of the theory of radiant pressure to all kinds of undulatory motion is based on the conservation of the energy. It remains to consider the mechanical origin of the pressure. In the special case of an unlimited stretched cord carrying transverse waves the advancing reflector may be a lamina, through a small hole in which the chord passes without friction: the cord is straight on one side of the lamina, and inclined on the other side on account of the vibration; and it is easily shown that the resultant of the tensions on the two sides provides a force acting on the lamina which, when averaged, agrees with the general formula. In the case of an extended medium with advancing transverse waves, which are reflected directly, the origin of the pressure is not so obvious, because there is not an obvious mechanism for a reflector which would sweep the waves in front of it as it advances. In the æthereal case we can, however, on the basis of electron-theory, imagine a constitution for a reflector which will turn back the radiation on the same principle as a metallic mirror totally reflects Hertzian waves, and thus obtain an idea of how the force acts.

The case of direct incidence has here been treated for sim-

[* Ordinary electrodynamic theory based on electrons neglects $(v/c)^2$.]

plicity; that of oblique incidence easily follows; the expression for the pressure is reduced in the ratio of the square of the cosine of the angle of incidence. If we average up, after Boltzmann, for the natural radiation in an enclosure, which is incident equally at all angles, we find that the pressure exerted is one-third of the total density of radiant energy.

Adiabatics of an enclosed Mass of Radiation, and resulting General Laws.—Now consider an enclosure of volume V containing radiant energy travelling indifferently in all directions, and of total density E; and let its volume be shrunk by δV. This requires mechanical work $\frac{1}{3} E\delta V$, which is changed into radiant energy: thus

$$EV + \tfrac{1}{3} E\delta V = (E - \delta E)(V - \delta V),$$

where $E - \delta E$ is the new density at volume $V - \delta V$. This gives $\frac{1}{3} E\delta V = V\delta E$, or $E \propto V^{-\frac{4}{3}}$.

As already explained, if the original state has the constitution as regards wave-lengths corresponding to a temperature T, the new state must correspond to some other temperature $T - \delta T$. Thus we can gain work by absorbing the radiation into the working substance of a thermal engine at the one temperature, and extracting it at the other; as the process is reversible, we have by Carnot's principle

$$\tfrac{1}{3} E\delta V/EV = -\delta T/T,$$

so that $T \propto V^{-\frac{1}{3}}$.

Thus $E \propto T^4$, which is Stefan's law for the relation of the aggregate natural radiation to the temperature.

Moreover the Doppler principle has shown us that in the uniform shrinkage of a spherical enclosure the wave-lengths diminish as the linear dimensions, and therefore as $V^{\frac{1}{3}}$, or inversely as T by the above result. Thus in the radiations at different temperatures, if the scale of wave-length is reduced inversely as the temperature the curves of constitution of the radiation become homologous, *i.e.* their ordinates are all in the same ratio. This is Wien's law.

These relations show that the energy of the radiation corresponding to the temperature T, which lies between wavelengths λ and $\lambda + \delta\lambda$, is of the form $\lambda^{-5} f(\lambda T)\,\delta\lambda$. The investigation, theoretical (Wien, Planck, Rayleigh, etc.) and experimental (Lummer and Pringsheim, Paschen, etc.) of the form of this function f is perhaps the most fundamental and interesting problem now outstanding in the general theory of the relation of radiation to temperature. The theoretical relations on which this expression is founded have been shown to to be in agreement with fact; and it appears that the form $c_1 e^{-c_2/\lambda T}$ fairly represents $f(\lambda T)$ over a wide range of temperature.* These relations have been derived, as usual, from a dynamical discussion of the aggregate intensity of radiation belonging to the temperature; it may be shown that the same results, but nothing in addition, will be gained by applying the same principles to each constituent of range $\delta\lambda$, by itself, assigning to each its own temperature.

[* Planck's more recent form $c_1/(e^{c_2/\lambda T} - 1)$ applies over a wider range in λT.]

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THE RESULTS OF AN ELECTRICAL EXPERIMENT, INVOLVING THE RELATIVE MOTION OF THE EARTH AND ETHER, SUGGESTED BY THE LATE PROFESSOR FITZ GERALD.

By Fred. T. Trouton, D.Sc., F.R.S.

[From the *Scientific Transactions of the Royal Dublin Society*, April 1902.]

I.—Professor Fitz Gerald's Arrangement.

In the Autumn of 1900 Professor Fitz Gerald proposed an electrical experiment, with the object in view of detecting any relative velocity there might be between the Earth and ether.

The method has not up till now been published, except at a meeting of the Dublin University Experimental Science Association last May.

Professor Fitz Gerald asked me to carry out the proposed experiments. These I began at once, but owing to delays in preparing the apparatus, and getting it into working order, only some preliminary determinations were made before his illness and death.

The fundamental idea of the experiment is that a charged electrical condenser, when moving through the ether, with its plates edgeways to the direction of motion, possesses a magnetic field between the plates in consequence of its motion, in accordance with the generally held view that a moving charge is equivalent to an electric current.

The question then naturally arises as to the source supplying the energy required to produce this magnetic field. If we

attribute it to the electric generator, say a battery,* there is no difficulty indeed, as to there being energy enough to do it, for, in general, the energy supplied by a battery when charging a condenser is double that stored in electrostatic strains in the condensor—

$$EQ = \tfrac{1}{2}EQ + \text{energy lost as heat, etc.}$$

Fitz Gerald's view, however, was that it would be found to be supplied through there being a mechanical drag on the condenser itself at the moment of charging, very similar to that which would occur were the mass of any body situated on the surface of the Earth to suddenly become greater. Again, in discharging the condenser should experience an impulse of like amount, but now in the opposite or forward direction. To estimate the extent of this blow, suppose a condenser of capacity P to be moving edgeways through the ether with the velocity u, and that the density of its charge is σ. Then the magnetic force between the plates which has to be established is $H = 4\pi\sigma u$.† The energy which must be provided to do this is $T = \frac{8\pi}{\mu} H^2$ × (volume of dielectric). Remembering that $\sigma = \frac{KE}{4\pi c}$ where K is the specific inductive capacity, E the voltage between the plates, and c their distance apart, and also that the capacity $P = \frac{K}{4\pi c^2}$ × (volume of dielectric), we have $T = \frac{1}{2}\mu KPE^2u^2$.

We had at our disposal a capacity of about 8 mf., which I found was able to stand 1200 volts. When charged with this voltage, on the above supposition, it comes out that it requires about 1 erg to energize the magnetic field when moving edgewise through the ether at the velocity of the Earth round the

* See Part II. of this Paper.

† The moving positive and negative charges on the two plates, that is, moving relatively to the ether, are equivalent to currents running tangentially in opposite directions in the two plates, so that we may take the field as existing only between the plates. If the plates move "flatways," the equivalent currents are in the normal directions, and neutralize each others' magnetic action almost completely.

Sun, that is at 19 miles per second; this, in centimetres per second, being $u = 10^6$ about. As regards the other quantities we may take $\mu = 1$, and for the specific inductive capacity of the paraffined paper of which the condenser was constructed, we may take 2 as a probable estimate, or in electromagnetic measure $K = 2/v^2$, v being the velocity of light and equal to 3×10^{10} cm. per second.

Thus we see that, on charging such a condenser, when placed so as to have its plates edgewise to the direction of motion of the Earth round the Sun, it must get from some source or other 1 erg more energy supplied it than it requires when moving flatwise, unless indeed the ether be in some way dragged along with matter and there be no magnetic field. Fitz Gerald's supposition was, as we have seen, that this came through the condenser receiving a compensating forward jerk or impulse on being charged, transmitted from the Earth through the supports; and he proposed to detect it by suspending the condenser at the end of an arm with a balance-weight on the other side, by means of a wire, as shown diagrammatically in fig. 1 (see p. 560).

It was originally intended to have two condensers, one at each end of the cross arm, the one to be charged at the moment the other was discharged, not only to double the effect, but also to secure a pure torque acting on the wire. This idea had to be abandoned in the final experiment, owing to all the condensers available breaking down under the excessive voltage employed save only one. A condenser similar to the one used for charging was employed as the balance-weight so as to preserve symmetry as far as was possible.

As the condenser plates lay horizontally, the effect should have been a maximum at twelve o'clock. The experiments were therefore carried out between the hours of eleven and one each day. In order to have the maximum turning effect the arm was placed north and south. Some preliminary experiments were made with a view of testing if a blow, whose kinetic energy was only 1 erg, could be competent to produce observable effects.

This was done by causing a small object to strike against the arrangement.

These proving highly encouraging, apparatus was constructed with the object of enabling the condenser to be charged and discharged continuously by means of clock-work, at the intervals corresponding to the free period of swing of the apparatus. In this way any effect produced would cumulate and be made easier of observation.

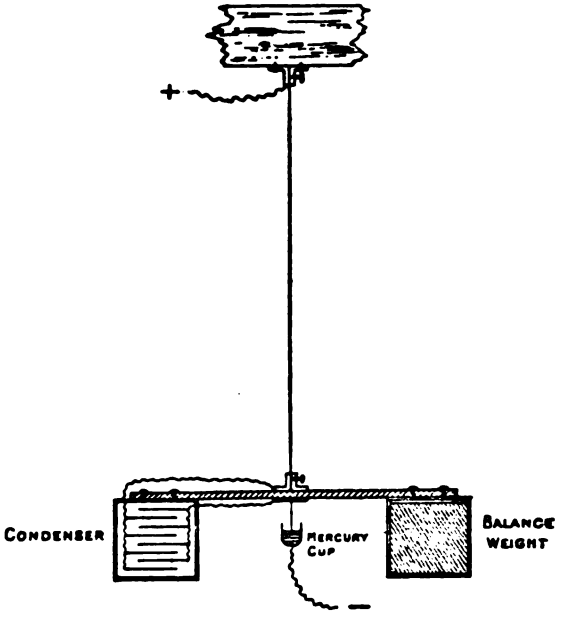


FIG. 1.

The complete period of swing was about 60 seconds, so that the charging and discharging followed each other at half these intervals or about 30 seconds. Coincidence between the rate of the clock which drove the commutator and that of the apparatus was most readily effected by increasing or diminishing the moment of inertia of the suspended apparatus, by means of small weights laid on it or removed as required. This coincidence in period was made very perfect.

The source of the current for charging the condenser was a continuous-current dynamo separately excited, capable of supplying a current of $\frac{1}{2}$ ampère at 1200 volts. Relays were employed for charging and discharging the condenser, as it was found undesirable, being likely to affect the rate of the clock, to allow such heavy sparking as necessarily took place to occur on the clock-driven commutator.

The leads for the charging current were provided by the suspending wire and by a wire which dipped centrally downwards into a mercury cup. A curved mirror was affixed to provide a spot of light on a mm. scale at the distance of 1860 mm.

The method of experimenting was to charge and discharge at proper intervals by means of the synchronous commutator, and to observe if a swing was set up. Another plan sometimes used was to cause the apparatus to have a small swing to begin with, and to see if its rate of decrement was increased by the effect in question when properly timed to do so. No effect was ever observed.

The Sensitiveness of the Apparatus.

The method adopted to test the sensitiveness of the apparatus to small quantities of energy, supplied synchronously with its own period, was to place a small magnet on the suspended apparatus at its centre, and to arrange that an intermittent magnetic field was synchronously applied for half the period of swing. The field was applied by means of a circuit of wire placed parallel to the magnet and at one side.

To ascertain the minimum magnetic force competent to set up a swing, the procedure was to gradually reduce the current in this circuit of wire until no effect could be observed on the apparatus at rest, the current all the while being applied at the proper intervals. From this a maximum limit to the value to be assigned to the minimum energy capable of producing a swing may be determined, by calculating the energy given per swing by this minimum field, when the apparatus had reached

a swing clearly observable, because when it was swinging through all angles less than this the energy supplied must have been proportionally less. When the suspended apparatus swings through an angle θ, the energy supplied is $MH\theta$, where M is the moment of the magnet, and H the strength of the applied magnetic field. The minimum magnetic field was found to be about $H = \cdot 037$ when employed with a magnet whose moment was $M = 204$; and taking that a movement of the spot of light of 2 mm. could be detected, we get for the angle θ the value $\frac{1}{2} \times 2/1860$, the scale being 1860 mm. from the mirror. This gives for the energy supplied once in each complete oscillation $MF\theta = \cdot 004$ ergs about. That is to say, we may safely assert that ·004 ergs is competent to set up a swing.

We have seen, however, that the energy necessary to generate the magnetic field assumed to exist is as great as one erg, and this was applied and removed in the experiments each half oscillation; so that it is evident that some other source for the energy or some countervailing effect must clearly be looked for.

On the last opportunity I had of discussing the matter with Professor FitzGerald, preliminary experiments had been made, giving as far as they went negative results: the final results not being completed till after Science had to deplore the grievous loss it sustained at his death. FitzGerald, on that occasion, made a remark which, as well as I remember, was to the effect that should the negative results then obtained be sustained by further work, he would attribute the non-occurrence of any observable effect to the same general cause as produced the negative results in Michelson and Morley's experiments on the relative motion of the Earth and the ether by means of the interference of light.

The explanation of this, which he had himself, simultaneously with Lorentz, suggested, was that matter altered in linear dimensions according to the direction of motion through the ether, and to an extent to just neutralize the calculated optical effect. From some such cause a diminution of the electrostatic

energy might be brought about when the condenser was in the edgewise position, just sufficient in amount to provide the energy required for the magnetic field.

II.—On the Possibility of obtaining a turning Force from the Motion of the Earth through Space.

If we suppose the energy for the magnetic field between the plates of a charged condenser when moving "edgewise" through the ether to be supplied from the same source as that which charges the condenser, we are led to the curious conclusion that a charged condenser tends to set itself flatways to the direction of motion through the ether. This may be easily understood from the following considerations.*

A condenser charged when moving "edgewise" has more energy to be supplied it than when moving "flatwise." Now suppose a condenser which is placed with its plates "flatwise" to the motion through the ether, to be first charged and insulated, then turned round till it is standing with its plates "edgewise" to the motion through the ether. The extra energy belonging to the magnetic field, which we suppose to now exist, must come from work done in turning it round through the 90°. That is to say, the condenser resists with, or exerts, a couple in the opposite direction to the turning motion.

Thus we see, on the assumption made above, that charged condensers must tend to set themselves at right angles to the direction of motion through the ether. When placed exactly "edgeways" to the motion, there is unstable equilibrium; but when inclined to either side, rotation to the position at right angles tends to take place indiscriminately as to the sign of the charges on the plates. This follows at once from the energy of the magnetic field depending on the squares of the directed quantities in question. The value of the torque increases with the angle of inclination, till it reaches its maximum half-way between the two positions of "edgeways" and right angles to

[* See No. 108 *infra*.]

the motion through the ether, at each of which it is zero. For if ϕ is the angle of inclination of the plates of the condenser to the direction of motion through the ether, the velocity of flow parallel to the plates is $u \cos \phi$. Thus the energy of the magnetic field is in this case $T = \frac{1}{2}\mu KPE^2u^2 \cos^2 \phi$, and the couple at any angle ϕ is $-\frac{dT}{d\phi}$ or $\frac{1}{2}\mu KPE^2u^2 \sin 2\phi$, this obviously has zero value for values 0° and 90°, and a maximum value for $\phi = 45°$.

From this point of view, instead of a translatory impulse as suggested by FitzGerald, it is a purely turning effect which should be looked for, and the author proposes to test the matter by suspending a light condenser of very high insulation, with its plates standing vertically. As the effect increases with the square of the voltage, it is advantageous to have this quantity as large as possible. This may be the more readily done in the present case, as the same complications do not present themselves from sparking, etc., as arose where intermittent charging was necessary in the experiments described in the body of this paper. By having the suspended apparatus light, the moment of torsion of the wire may be greatly reduced, which is of the highest importance.

Should this turning moment be proved to operate, instead of being masked by some compensating effect, it would open up a road leading to illimitable possibilities, for it would at once remove from the category of utter hopelessness the idea of mankind ever being able to utilize the vast store of energy in the Earth's motion through space.

It is not difficult in theory to conceive a machine for doing this, in fact for harnessing the Solar system, so to speak. For instance, a number of air condensers could be arranged round a cylinder so as to be capable of rotating as a whole about its axis, and spaced with their plates tangentially to their circular motion, the axis of rotation being always kept at right angles to the motion through the ether. Now, suppose one of these condensers to be charged with electricity. As it moves up from

the "edgewise" position to the "flatwise" position, there is a couple exerted, turning the whole apparatus round the common axis. On reaching the latter position, the charges on the plates of the condenser are to be conveyed to another of the condensers, which, in the course of its passage round, has just reached the "edgewise" position vacated by the first one in question. This must be effected without sparking, or there would be an undue loss of energy. This might be done by a method very similar to Maxwell's well-known "regenerator" arrangement. As each condenser comes to the "flatwise" position, it enters an inductor arrangement, one for each plate of the condenser, and when fully inside each plate touches a contact spring. Simultaneously a pair of condenser plates at the "edgewise" position is emerging from a pair of inductors, one for each plate, but still kept in metallic contact with the inductors by springs. The inductors in each position are in metallic connexion with the corresponding inductors at right angles. In this way the charge is removed from one set of plates and given to the other. The same could be also used in duplicate on the diametrically opposite position, thus giving double the turning force.

I take this opportunity of thanking Professor J. Larmor, Sec. R.S., for his advice and helpful discussions of the subject-matter of this paper.

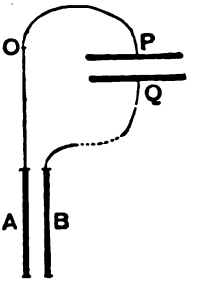
108

CAN CONVECTION THROUGH THE ÆTHER BE DETECTED ELECTRICALLY? NOTE ON THE FOREGOING PAPER.

By J. Larmor.

The considerations explained by Dr. Trouton in the preceding paper bear fundamentally on the question whether the convection of matter through the æther sensibly disturbs that medium. The result of the experiment which he proposes to carry out will either corroborate or modify the results deduced from the Michelson-Morley second-order experiment on optical interference, the interpretation of which has hitherto been somewhat ambiguous because that experiment has stood alone as the only one of its kind.

We begin by tracing the theoretical consequences of an ideal experimental arrangement. Consider a condenser AB suspended from a point O, and charged (or rather its charge altered) from another fixed charged condenser PQ in the same circuit. Separating the plates P and Q increases the charge in AB, and *vice versa*. Now charge AB when its plates are perpendicular to the direction of the æther drift; then disconnect it and rotate it through a right angle. Finally, let AB be discharged by re-connecting to PQ—whose plates have meantime been moved to the distance that will give them the same difference of potential as AB—and then approximating the plates P and Q to their original distance. After this AB may again be rotated through a right angle when it is uncharged, and the cycle repeated. The conductors being supposed perfect, this will give a perpetual motion, or rather will use up the energy



of the Earth's translatory motion through the æther,—unless the potential difference between A and B, on which the energy of charge and discharge depends, is the same in the two positions, for a given value of the charge. This potential difference is sustained, in part by the æther strain, in part by the magnetic field between the plates which is produced by the electric convection

Now a known electrodynamic result* may be stated as follows:—Compare an electrostatic conducting system at rest in the æther with the identically same system in motion with velocity v; suppose the charges to be the same in the two cases: to obtain the potential differences between the conductors in the latter case, find what they would be in the system when at rest, and elongated in the ratio $\epsilon^{\frac{1}{2}}$ or $\left(1-\frac{v^2}{c^2}\right)^{-\frac{1}{2}}$ in the direction of the motion, and multiply the result by ϵ. Let us apply this proposition to the present case.

(i) When the plane of the condenser is at right angles to the drift, the thickness of the dielectric in the correlative one at rest is increased $\epsilon^{\frac{1}{2}}$ times, thus so is the potential difference for given charge, and the other factor ϵ makes the increase $\epsilon^{\frac{3}{2}}$ times in all. (ii) When the plane of the condenser is parallel to the drift, the plates are each elongated $\epsilon^{\frac{1}{2}}$ times; hence the density on them is increased $\epsilon^{-\frac{1}{2}}$ times; the dielectric thickness being unaltered, this increases the potential difference $\epsilon^{-\frac{1}{2}}$ times, and the other factor makes the increase $\epsilon^{\frac{1}{2}}$ in all.

Thus position (i) of the condenser has ϵ times as much electric energy as position (ii) for the same charge Q; that is, it has more energy than (ii) by $\frac{v^2}{c^2}$ of the total energy of charge in either position, this latter energy of charge being $2\pi\frac{Q^2}{S}t$, where t is the thickness of the dielectric, and S the area of either plate.

In position (ii) there is a magnetic field between the plates, of intensity $4\pi\frac{Qv}{Sc}$, having electrokinetic energy $\frac{1}{8\pi}\left(4\pi\frac{Qv}{Sc}\right)^2 St$, or $\frac{v^2}{c^2}2\pi\frac{Q^2}{S}t$, which forms part of the electric energy of the

* Larmor, 'Æther and Matter,' 1900, § 96.

system, the energy of æthereal strain being correspondingly diminished; on rotation of the condenser through a right angle this kinetic energy disappears.

Now the condenser is charged by transferring the charges into the plates against the electric force; the energy required for this operation is half the charge Q multiplied by the potential difference between the plates. As it is charged at one potential difference and discharged at another, there is energy remaining over of the amount estimated above; and as the process is reversible, this energy must be mechanically available. Thus the energy of motion of the Earth through the æther is available for mechanical work to an unlimited extent, unless the potential difference in the condenser is independent of its orientation; that is, by accepted electrodynamics, unless the FitzGerald-Lorentz shrinkage of moving bodies is a fact.

It is to be observed that the proposition that, if this shrinkage occur, the system will in no other respect differ from the state of no motion, is established for uniform translatory motion; so that a torque arising when the motion is rotatory would not be in contradiction to it. But such a torque must depend on the velocity of rotation, vanishing along with the latter, and so does not affect the present argument in which the displacements can be made very slowly.

As the electric energy of the condenser is reduced by turning it from the transverse to the longitudinal position, this motion would, in the absence of the FitzGerald-Lorentz shrinkage, be assisted by a mechanical torque, of amount $\frac{v^2}{c^2} W \sin 2\theta$, where W is the energy of charge, and θ is the angle the plane of the condenser makes with the transverse position*; thus the transverse position would be one of unstable equilibrium.

* The torque is equal and opposite to that suggested by the considerations given on p. 563. For the condenser described on p. 562, suspended vertically at an inclination of 45° to the æther-drift, and charged as in the experiments there recorded, the value of this torque would be about 1 c.g.s. unit; while the torsional system as there adjusted was found to respond sensibly to about 7·5 c.g.s. units.

For the case of a single charged conductor, such as may be represented by an ellipsoid, it is not difficult to show that the stable orientation is the one in which its longest dimension is along the direction of convection.

The charge and discharge of the condenser involve currents round the condenser AB, and round the discharging circuit. So long as the suspending wire OA does not move with the rotation, and also the discharging wire BQ, the Earth's magnetic field can produce a torque only by acting on the charging current circulating round AB,—for charging A positively and B negatively is the same as making a current flow down A and up B, if both plates are charged from the top. This torque is negligible in the very slowly effected cycle of the experiment; but any uncertainty can be obviated by reversing its direction each time by an arrangement which first annuls, and then, after half a period, reverses, the charge of the condenser AB.

If the condenser AB is held *absolutely fixed* while it is being charged, any impulsive torque there might be could do no work; yet the condenser gets its energy. This seems by itself sufficient to negative the suggestion that the energies of charge and discharge, as distinct from that derived from rotating the condenser, have to do directly with mechanical forces.

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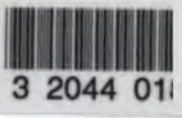

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